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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—PHYSIOLOGICAL PAPERS. No. I.—*Observations on the Mechanism of the Ear.* By JOHN G. M'KENDRICK, M.D., F.R.S.E., Physiological Laboratory, University of Edinburgh.

THE vibration of the membrana tympani corresponding to the ibration of the air constituting various sounds, is the first element in the transmission of sound to the internal ear. This membrane is not only placed obliquely at the bottom of the external meatus, but its surface is curved inwards or outwards, according to the relative amount of atmospheric pressure in the tympanum and in the external meatus. As the margins of the membrane are firmly fixed to the tympanic ring, and as its middle portion, attached to the handle of the malleus, is either drawn inwards or pushed outwards, the structure is in a very tense condition, like that of a tightly-strung drumhead. Any one may be convinced of this fact by examining the membrane in a dissected preparation of the ear of any mammal, even after the disappearance of rigor mortis. Consequently any point on the membrane, when the structure vibrates by a wave of sound, will move only through a very short distance—a distance very much less than the wave-length of the sound producing the displacement. The wave-length of any given musical note is found by dividing the velocity of sound in air by the number of vibrations per second producing the note. Taking the velocity of sound in air, at the temperature of 60° F., as 1080 feet per second,[1] we thus obtain the wave-length in the immediate vicinity of the sounding body. But the wave-lengths decrease in the ratio of the squares of their distance from their origin. For example, if the wave-length of a note close to the sounding body be 2 feet, at a distance of 2 feet it will be 1-4th of 2 feet; at a distance 3 feet, it will be 1-9th of 2 feet; of 4 feet, 1-16th of 2 feet, and so on. The greater the distance, therefore, of the sounding body from the ear, the less will be the effect on the membrane of the drum. It is impossible to calculate beforehand the extent of the oscillations of a point on a stretched membrane which may be caused by sounding any particular musical note near it. There are special difficulties (chiefly relating to form and to degrees of tension) in the way of doing so in the case of the membrana tympani.[1] The only method open is direct experiment. The extent of the excursion of an illuminated vibrating point on the inner surface of the membrane can, by proper arrangement, be measured. This has been done by several experimenters,[2] including Helmholtz himself; and although there are several differences in their measurements, they still afford information of the greatest importance. Helmholtz has given us data, derived from experiment, by which, if we know the amplitude of the excursion of points on the membrana tympani, we can calculate that of the excursion of points on the handle of the malleus, on the stapes, and on the membrane of the round window (fenestra rotunda). He has shown—(1) That the extent of movement of the membrana tympani as a whole is at least three times that of the handle of the hammer. This arises from the peculiar form of the membrane. (2) That the extent of movement of the process of the incus bearing the stapes, and of the stapes itself, is two-thirds of that of the handle of the hammer. (3) That the extent of movement of the stapes is about equal to that of the round window. We must now abandon the statement that the propagation of sound through the bones of the tympanum to the labyrinth must be effected by undulations of condensation and rarefaction of their particles only, and not by oscillations of the entire bones.[3] The objections to this view are at least the following two:—(1) The length of the chain of bones is only a small fraction of the wave-length of the notes readily detected by the human ear; and (2) if such a small compound body as the chain of ossicles were set into rapid vibrations, tones would be produced so high as to be inaudible.[4] The length of the chain of bones is about nine millimetres. Compare this with the wave-lengths of the following notes which I have calculated:—

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[1] Airy on Sound and Atmospheric Vibrations with the Mathematical Elements of Music. 1871. P. 140.

	Name of Note in Musical Scale.	Vibrations per second.	Wave-length, in millimetres.
	c_1	33	10,000
	c_3	132	2,500
In treble stave,	c_4	264	1,250
	F	352	937
	B	495	666
	c_6	1056	312
	c_8	4224	80 nearly.

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[1] Helmholtz, Mechanism of the Ossicles of the Ear, p. 53 *et seq.*

[2] Ibid. Pflüger's Archiv. f. Physiologie, i. Buck, Knapp and Moos's Archives, vol. i. 1870. Burnett, Knapp and Moos's Archiv., vol. ii. No. 2, p. 45.

[3] Müller's Elements of Physiology, trans. by Baly, vol. ii. p. 1255.

[4] Helmholtz, ibid. As to wave-motion, see Mr Sedley Taylor's "Sound and Music, a Non-Mathematical Treatise on the Physical Constitution of Musical Sounds and Harmony, including the chief Acoustical Discoveries of Professor Helmholtz." London: 1873.

The mode of action of the bones of the ear, therefore, is not to convey across the tympanum sonorous vibrations, as such, but to convey to the fluid of the labyrinth the impulses communicated from the membrana tympani, and the mechanical arrangement is such as to reduce the extent of movement of the stapes to at least two-thirds of that of the handle of the malleus. As has been pointed out by Helmholtz, the chain of bones is a lever of the second order, having the resistance between the fulcrum and the power. The fulcrum is the process of the incus which abuts on the wall of the tympanum; the resistance is at the stapes attached to the other process of the incus; and the power is exerted on the handle of the malleus. The distance from the fulcrum to the point of resistance is just two-thirds of the distance from the fulcrum to the power, and consequently the extent of movement at the point of resistance (the stapes) is two-thirds of that at the handle of the malleus, where the power is applied. Thinking over these facts, and others which have come under my own observation in repeating Helmholtz, Buck, and Burnett's experiments, it has occurred to me to attempt to calculate the amount of movement which a point on the stapes, or the stapes itself, would perform, if impulses were communicated to it from the membrana tympani set in vibration by very high musical tones. There are experimental difficulties in the way of observing vibrating points on the stapes while performing very minute excursions. It is difficult to obtain sufficient illumination to examine the point with high microscopic powers. Burnett succeeded in five experiments in observing excursions on the head of the stapes produced by an organ pipe giving 1160 vibrations per second, and in one experiment he failed. The amplitudes of excursion observed were thousands of a millimetre. Excursions on the membrana tympani can be seen more readily. The calculations which I have made are only approximations; but they show the extent to which the motion implied in the wave-length of a musical note in air is reduced by the mechanism of the middle ear. I have proceeded as follows: Taking the average amplitudes of the excursions at the point of the long process of the hammer, I have assumed, on the one hand, the correctness of Helmholtz's statement that the mean of the motion of the membrana tympani is three times as much; and on the other the evident fact (on the lever theory) that the movement of the stapes is two-thirds of that of the handle of the malleus. In the case of those tones which produce movements so slight that as yet we have not surmounted the experimental difficulty of observing them, I have also assumed that the membrane and handle of the hammer would vibrate to a smaller extent in direct proportion to an increase in the number of vibrations producing the tone, and I have calculated accordingly. This, of course, is so far an erroneous basis on which to found a calculation, because membranes do r-- vibrate in such a regular manner. Still, with notes of the intensity but of different pitch, the excursions of a point on a vibrating membrane will be greater for a note of low than for a note of high pitch, and intermediate notes will occupy nearly certain definite points in the scale. In the meantime, therefore, until direct observation has been made of the vibrating point, I have hazarded the assumption. The following table shows the results in fractions of a millimetre:—

Note produced by	Mean displacement of membrana tympani.	Excursions at point of handle of malleus.	Excursions at the stapes.
50 vibrations p. second	$\frac{2}{25}$	$\frac{1}{25}$	$\frac{1}{42}$
132 "	$\frac{9}{100}$	$\frac{3}{100}$	$\frac{1}{50}$
264 "	$\frac{9}{200}$	$\frac{3}{200}$	$\frac{1}{100}$
352 "	$\frac{32}{1000}$	$\frac{11}{1000}$	$\frac{11}{1500}$
630 "	$\frac{1}{42}$	$\frac{1}{125}$	$\frac{1}{188}$
1,160 "	$\frac{1}{83}$	$\frac{1}{250}$	$\frac{1}{375}$
4,224 (highest note used in ordinary music)	$\frac{3}{1000}$	$\frac{1}{1000}$	$\frac{1}{1500}$
6,000 "	$\frac{9}{5000}$	$\frac{3}{5000}$	$\frac{1}{2500}$
8,000 "	$\frac{1}{666}$	$\frac{1}{2000}$	$\frac{1}{3000}$
15,000 "	$\frac{1}{1666}$	$\frac{1}{5000}$	$\frac{1}{7500}$
38,000 (highest audible sound)	$\frac{3}{10000}$	$\frac{1}{10000}$	$\frac{1}{15000}$

It thus appears that the amplitude of the excursions of the stapes when produced by even the highest notes is enormous as compared with the wave-lengths of light which affect the retina. This will be at once seen by comparing the figures given above with the following table, which shows the wave-lengths of the mean rays of the several colours of the spectrum in fractions of a millimetre:[1]—

Colour.	Wave-length in ten-millionths of a millimetre.
Red	6·20
Orange	5·83
Yellow	5·51
Green	5·12
Blue	4·75
Indigo	4·49
Violet	4·23

If, therefore, there be any analogy between the ultimate action of sound on the auditory terminal apparatus, and that of light on the retina, the arrangements in the vestibule, canals, and cochlea must be such as still further to reduce extent of movement. The fluid in the labyrinth is surrounded by resisting walls, and is practically incompressible. But, as Helmholtz indicates, a single shock communicated to such a fluid contained in a confined space is communicated through it in every direction many times in an inconceivably short space of time. Thus the comparatively extensive excursions of the stapes affect the fluid in the labyrinth, and the delicate thrills so produced may act on the vibratile structures in the vestibule or on the rods of Corti, and so cause impulses quite as minute as those of the waves of ether impinging on the retina. A highly educated ear can detect many variations in sound, just as an educated eye can distinguish many slight shades of colour, and the one organ is probably as finely constituted as the other. There is this difference between them: waves of light do not require to be reduced in amplitude before they impinge on the retina, and, therefore, there are only refractive structures through which they must pass so as to be brought to a focus. The total amount of energy in light is not reduced, except to an infinitesimal degree. But, in the case of the ear, there is a complicated apparatus for reducing extent of movement. Only a small fraction of the total amount of energy of waves of sound reaches the delicate organs in the depths of the internal ear.

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[1] Article on Light by Roscoe, in Watts's Dictionary of Chemistry, vol. iii. p. 600.

ARTICLE II.—*On a Cause of Retention of Urine in Fibrous Tumour of the Uterus, with Cases.* By J. R. HARDIE, M.B., etc.

THE intimate anatomical relation of the uterus with the bladder and urethra is brought forcibly to the mind of the gynæcologist when considering the symptomatology of various pathological conditions of the former organ. In fact, there are few diseases of the uterus in which urinary symptoms do not occasionally present themselves in a greater or less degree. The presence of a fibrous tumour in the walls of the uterus frequently—not always, for its existence may be accompanied by no symptoms whatever—gives rise to urgent and distressing bladder symptoms.

In this short paper I do not purpose entering into an examination or even an enumeration of all the symptoms connected with the bladder which may exhibit themselves during the progress of the growth of a fibroid of the uterus, but shall limit myself to a consideration and attempt at explanation of one of these, namely, a sudden and complete temporary retention of urine. This ordinarily occurs, as I maintain, immediately before the advent of a monthly period, or during the first days of its duration.

The explanation which is about to be offered to account for this retention is a purely mechanical one. It is, that the fibroid, participating in the general hyperæmia to which the internal genital organs are liable when ovulation takes place, becomes congested with blood, and so increases in bulk that it exerts a pressure force on the neck of the bladder or urethra, which is sufficient, when at its acmé, for a time, to render unavailable any attempts to empty the bladder naturally. That an increase in the size of some fibrous tumours of the uterus does take place during menstruation

I have verified by actual measurement in two cases, where the tumours were of large size and occupied the abdomen, being thus readily accessible for the purpose. The slow rate at which simple fibroids of the uterus usually grow may account for the fact that complete retention of urine is not more common in cases where they exist. The neighbouring organs have time to accommodate themselves to its gradually-increasing bulk and pressure, and instead of complete retention, partial retention of urine, or difficulty and pain in micturition, are the more frequent symptoms. But during menstruation we have a rapid increase of size of the tumour, and corresponding increase of pressure, to consider—a pressure which the neighbouring organs are called upon to bear suddenly, and for which they are not prepared.

When in such cases of retention of urine the fibroid is of large size, and occupies the cavity of the pelvis, the cause of its occurrence now proposed seems a natural one, and may be readily accepted. When, however, it is of small size—about the size of a hen's egg, for instance—difficulties may arise in the way of its acceptance; but even then, more especially when it is situated in the anterior wall of the uterus, the resistance which the uterine walls themselves offer to its extension upwards and backwards, may suffice to create a counter-pressure, the force of which, when directed against the urethra or neck of the bladder, is sufficient to cause retention of urine.

The amount of pressure-force which it is necessary to bring to bear on the urethra or neck of the bladder in order to arrest the flow of urine through them, or, what comes nearly to the same thing, the amount of force with which the urine may be expelled, has not, so far as I know, been accurately investigated, but, judging from rough experiments which I have witnessed, it is probably not great.

The small amount of force with which it is necessary to compress the urethra in order to cause retention of urine is well illustrated in those cases which sometimes occur after parturition, when the labour has been of more than ordinary difficulty. In such cases the soft parts in the neighbourhood of the vagina become swollen, and pressure so applied to the urethra. Considering the shortness of the urethral passage, and its almost straight direction, it is surprising that so little pressure is sufficient to overcome the expelling force of the bladder and abdomen.

When the tumours are large and occupy the abdomen, their relations to the bladder and urethra are altered, and a sudden increase in bulk, such as has been described, will not be so likely to give rise to retention of urine as when the tumours occupy the cavity of the pelvis. Although I have not collected cases to prove this point, yet clinical experience supports generally the statement here made. I here insert three cases illustrative of the question to which I have just drawn attention.

CASE I.—C. S., aged thirty-six years, unmarried, sent for me on the 24th of April 1871. On reaching her, I found that she complained of pain in the hypogastric region, and that she had not been able to pass her urine for eighteen hours, although strongly desiring to do so.

Previous History.—Five years ago, noticed that she began to lose more blood than usual at monthly periods, and had a white discharge during the intervals. For the last nine months has occasionally experienced difficulty in micturition, more especially when menstruation set in. It was generally most urgent during the first day of the discharge, and some relief was usually obtained after the lapse of from eighteen to twenty-four hours. Has hitherto never required to have her water drawn off by means of the catheter.

Present Condition.—Pain on pressure over the hypogastrium above the symphysis pubis. There is dulness on percussion from symphysis pubis to within an inch of the umbilicus. A catheter was now introduced into the bladder, and about forty ounces of urine evacuated. This operation afforded patient relief from pain, and dissipated the dulness in the hypogastrium.

Examination per vaginam.—A rounded smooth lump, about the size of a hen's egg, can be felt, situated above and in front of the cervix uteri; it is movable, and the uterus can be observed to move with it. Nothing else abnormal to be made out. At this stage of the examination I discovered that patient was menstruating; there was not much discharge. She said it had been present for four hours. On passing the sound on a subsequent occasion, the uterus was found to measure fully three inches.

25th April.—Menstruation profuse. Passed urine this morning.

23d October.—Was called to see patient under similar circumstances to those just related.

I am indebted to the kindness of Dr Matthews Duncan for access to the two following cases:—

CASE II.—A. F., from Glasgow, aged thirty-two years, and unmarried, has suffered from menorrhagia for the last nine months. Three months ago she found herself unable to pass water at the beginning of a menstrual period. She obtained medical advice, and was relieved by means of the catheter. At the succeeding monthly illness she experienced pain and difficulty in micturition, worst when the period was just coming on, and during the first day of its appearance. This passed off completely on the disappearance of the discharge. Since that time urination has been painless and easy.

Physical Examination, 21st August 1872.—On examination per hypogastrium, there can be felt a rounded, smooth tumour, about the size of an orange, projecting from the cavity of the pelvis. Attached to its right side is a nodule about the size of a boy's marble; it also is smooth and rounded. On examination per vaginam, this tumour is seen to be connected with the uterus, more especially with its fundus and anterior wall. Probe passes into the uterus three and a half inches.

A sound passed into the bladder discovers nothing abnormal there.

CASE III.—B. T., who is forty-four years of age, and unmarried, consulted Dr Matthews Duncan, 5th November 1873. He informs me that she suffered from a fibrous tumour, about the size of a fœtal head, connected with the posterior wall of the uterus, and that that organ was retroverted. This condition was rectified, and the tumour pushed above the brim of the pelvis. I obtained from her the following particulars of her case, which are of more immediate interest to us at present:—

She had enjoyed good health until two years ago, when she was attacked by menorrhagia, pain in the lower part of the belly, and difficulty in micturition. These symptoms have continued with greater or less intensity up to the present time. One morning last March, when menstruating, on rising to make water, she found that she was unable to do so; she returned to bed, suffered great pain in abdomen until the evening, when she succeeded in making water. She had similar attacks during the months of August and October, and met them in a similar fashion, dispensing with medical aid, always succeeding ultimately in passing water without the use of the catheter. Each of these attacks occurred during a monthly period, at which time, she says, the bladder symptoms are most severe, although they are more or less constantly present.

On a perusal of these cases, there are two noteworthy circumstances to be observed in connexion with the kind of retention under consideration in this paper; firstly, that the more urgent urinary symptoms occurred when the menstrual discharge was about to appear or had first set in,—this happened in the first two cases; secondly, that they passed off when the discharge had fully established itself, or during the first days of appearance, or on its disappearance. In the case of C. S., who was more immediately under my own care, these facts were well observed, the retention setting in a few hours before the menstrual flow appeared, and passing off during the first day of its duration. The first of these circumstances—namely, the setting-in of the retention of urine shortly before menstruation or at the commencement of menstruation—is probably due to the congestion at this time being at its height; while the latter circumstance—namely, the relief experienced after the flow is fairly established or during its first days—may be explained by the fact that the discharge of blood relieves the congestion of the tumour, lessening its bulk, and thus diminishing the pressure to which it gives rise.

In conclusion, I do not mean to contend that the explanation just offered to account for the retention of urine during menstruation, when a fibroid is present in the uterus, suffices to explain all such cases. On the contrary, I am convinced that there are other causes at work to produce this effect besides the one I have brought forward.

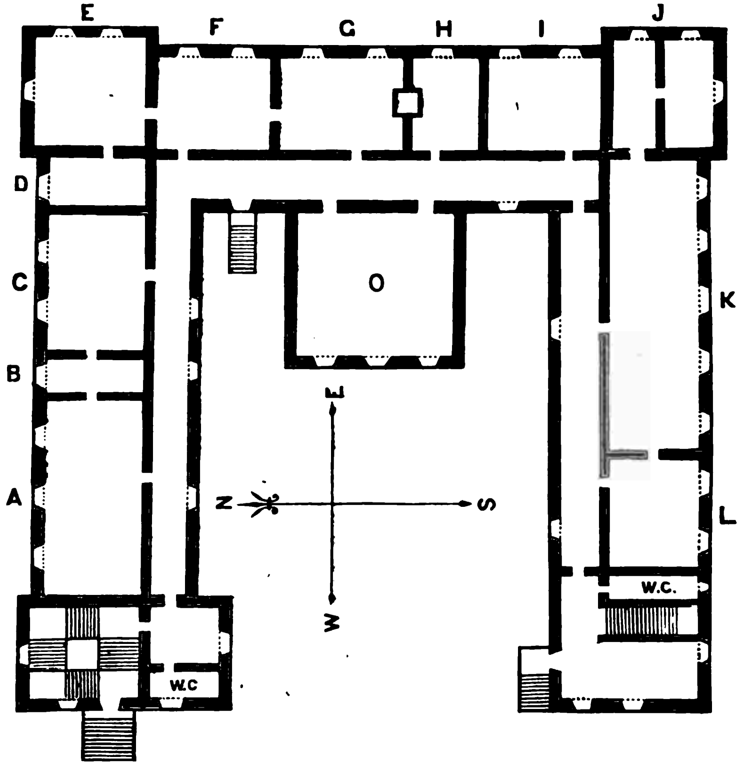
ARTICLE III.—*The School of Physiology at Leipzig.* By WILLIAM STIRLING, D.Sc., M.B., Demonstrator of Practical Natural History in the University of Edinburgh.

AT this School, up to the year 1865, Physiology and Anatomy were united in one chair, and taught by Ernest Heinrich Weber, with first Funke and then Hering as extraordinary professors. During the course of this year, Carl Ludwig, the present professor, was called from Vienna to lecture upon Physiology. At that time no separate institution for performing physiological experiments existed, but Ludwig, with Schweigger Seidel as his assistant, extemporized a provisional laboratory. The school of Physiology at Leipzig is one of the first in Germany, and owes its origin and fame to the energy of its talented professor.

The "physiologische Anstalt" is a handsome building, pleasantly situated, with the front facing the street, directed towards the north. Its general form is somewhat that of a horse-shoe, with a projecting portion in the centre, as will be seen from the accompanying diagram.

The length of the building, on the north and south sides, is 119 feet 2 inches, and its breadth 121 feet 11 inches. The wing facing the north is devoted to histological research; that to the south is the chemical department; while in the central part, looking east, experimental physiology and physiological physics are carried on. The projecting portion (O) in the middle is the lecture-theatre. The building is surrounded by a garden, with an aviary and a small fish-pond. In the latter, the frogs are kept during the summer. In the court-yard is a separate building, with one compartment fitted up as a stable; in a second there is a large operating table, and a hoist, for performing operations upon horses and other large animals; while the third room is used for keeping the rabbits, guinea-pigs, etc., which are to be experimented upon.

In the building itself, in the under floor is a workshop, fitted up partly for carpentering, and partly with appliances for working in iron. A small gas-engine, of about one-horse power, stands in an adjoining room, and drives the whole of the machinery and apparatus in the establishment. Here also is a centrifugal apparatus for separating the serum from the blood-corpuscles and fibrin of blood, the serum from the coagulum in lymph, etc. The apparatus is very simple, and consists of a series of iron tubes, open at one end and closed at the other, so arranged round a central axis that, when the apparatus is caused rapidly to revolve, these tubes, by virtue of the centrifugal force, assume a horizontal position,



with their mouths directed towards the central axis, and their bases directed outwards. A glass tube with a rounded bottom, containing the substance to be examined, and properly corked, is then placed in one of these tubes, and the apparatus caused to revolve rapidly by a belt from the fly-wheel of the engine. After a time the clear fluid is found uppermost, and the coagulum at the base of the glass vessel. Underneath also is a room solely for frogs, which are kept throughout the winter in stone troughs. Two other rooms are fitted up with iron cages for the dogs which are to be experimented upon. A porter lives in the under flat, and looks after the animals, and works as a carpenter when required. The other compartments underneath are used partly as cellars; while those immediately underneath the chemical department are fitted up for distilling purposes, and with furnaces, etc., for chemical operations.

The annexed plan shows the internal arrangement of the laboratory proper. The square projecting building O in the centre is the lecture-room, lighted from above and from the side, and capable of containing over one hundred students. The light can be excluded when experiments so require. Here also experiments upon animals, involving the use of artificial respiration, can be demonstrated to the students. The table from which the professor demonstrates his experiments runs upon a small railroad, so that each student has an opportunity of seeing what is going on. The north, or histological wing, is divided into a series of compartments. Room A is arranged for beginners in the study of histology. It is lighted from the side, and over thirty students can work conveniently at one time. There is a large ground glass tablet upon which the demonstrator illustrates his subject by means of coloured chalks. The room is hung round with diagrams and drawings of the structure of the various tissues and organs. Each student is provided with a small case, containing the requisite reagents used in histological investigations, and has, in addition, a small press where he keeps his instruments, etc. Histology is demonstrated in two-hour courses three times a week during the winter, and daily during the summer. The small compartment B is the private working-room of the assistant. Room C is for more advanced students, and for those carrying on special microscopical investigations under the direction of the professor. This compartment is provided with several egg-hatching machines, apparatus for boiling preparations in alcohol, evaporating closets, and an injection-apparatus whereby three different coloured fluids can be injected into an organ at once, and under constant pressure. D is the library, amply provided with physiological literature, and all the physiological journals,—German, English, and French.

In that portion of the building looking east physiology proper and vivisections are carried on. E is the professor's private room. Here no experiments are performed. The instruments, when not in use, are kept here in a series of presses arranged round the room. The remainder of this portion of the east front is divided into a series of rooms, so that an operator carrying on one kind of experiment does not interrupt another working at a different subject. In F, small animals, such as rabbits, are experimented upon. The shafting, driven by the gas-engine, runs along the rooms F and G, so that experiments involving artificial respiration are easily performed. Each of these rooms is furnished with operating tables, bellows for performing artificial respiration on curarized animals, and a kymograph elaborately arranged for writing blood-pressure, etc. Experiments upon digestion are also performed in F. On one side of this room a series of water-baths are arranged, so that any required temperature can be kept up for any length of time. Here also is an injecting apparatus, similar to that in the histological section, glass cases for apparatus, evaporating closets, etc. In G, animals of larger size, such as dogs, are operated upon. It is furnished similar to F. When electric batteries are used, they are placed in ventilated closets arranged round the room, so that the gases thereby generated do not pollute the atmosphere. A small compartment between this room and the next can be darkened for optical investigations. H is the weighing-room, provided with coarse and delicate balances. I is the quicksilver-room, here all manipulations involving the use of mercury are carried on. It is furnished with the mercurial pump, modified and improved by Ludwig and Geissler, for extracting the gases from blood, lymph, etc., apparatus for measuring the capacity of tubes used in gas analysis, etc. J is divided by a partition into two—the one half is furnished with delicate balances for the chemical section, while in the other, optical and delicate electrical experiments are carried on, and experimental physics demonstrated. The chemical section, K, is elegantly fitted up after the manner of chemical laboratories generally. Each student is provided with the necessary chemical reagents, and he has to provide himself with test-tubes, beaker glasses, etc., while the more expensive apparatus is furnished by the institution. L is the assistant's private working-room. The professor and his family, the chemical assistant, and the mechanic, live in the upper storey.

Professor Ludwig devotes the whole of his time to his pupils, and there are at present seven other gentlemen besides myself studying under his direction. Here many nationalities are represented,—German, Russian, Italian, English, American, etc.; but each one generally refrains from speaking his mother-tongue, being too anxious to acquire a knowledge of German. Each one investigates some particular point in physiology, and the results of the investigations are published annually, brochure-form, under the title, *Arbeiten aus der physiologischen Anstalt zu Leipzig*. The papers therein contained are first read before the mathem.-phys. Classe der K. S. Gesellschaft der Wissenschaften zu Leipzig, which grants a sum of money annually to aid in the publishing of the volume. Already seven of these volumes have been published. This money was left by J. A. Jablonowski, who endowed a foundation, the annual proceeds of which are devoted to aiding the publishing and illustrating of the transactions of the above society.

The professor has three assistants:—Dr Fleschig, in the histological department; Dr Drechsel, in the chemical; and Dr Kronecker, in the physiological and physical sections. A skilled mechanic, G. Salvenmoser, who accompanied the professor from Vienna, also lives on the premises. In connexion with the laboratory, a "physiologische Gesellschaft" was established a few years ago. It meets every second Saturday evening in the histological department, when the results of original and other papers are communicated. The transactions, however, are not published.

Dr Kronecker gives, once or twice a week, extra lectures upon some physiological subject, and under his direction the Saturday afternoons are devoted to practical work, with the various instruments used in physiological research.

Few institutions offer such valuable opportunities for the study of physiology, and this is heartily testified to by the large number of students, who flock to hear the lectures and work under the superintendence of its distinguished professor. During the winter session Professor Ludwig lectures upon the physiology of the nervous and muscular system and on the organs of special sense. In summer the physiology of the circulation, respiration, and digestion are treated of. It will thus be seen that the physiological laboratory here is all that can be desired, and is not surpassed by any in Germany. If any instrument is required it is at once purchased, for the annual grant from the Saxon Government is liberal and ample. The late king was a special patron of the University, and the present king, his son, also takes a deep interest in the progress and prosperity of the Leipzig University, and has promised his support in the granting of money for conducting on a liberal scale the prosecution of the natural sciences.

ARTICLE IV.—*Cases illustrating Diseases of the Ear*, under the care of J. J. KIRK DUNCANSON, M.D., C.M., M.R.C.P.E.

DISEASES OF THE AURICLE.

30th July 1872.—Miss W., æt. 18, has felt a hard lump on the back of the left auricle for the last two years. Two weeks since, she got wet and suffered from cold. Whilst in bed, on lying on the left side she felt the back of the ear painful. The pain increased, with a throbbing sensation. On examination, a bright red swelling about the size of a turkey-bean was seen behind the concha. Fluctuation was distincly felt. A free incision was made, which allowed the escape of a thimbleful of glairy purulent matter. The cyst walls were destroyed, and a stimulating lotion applied. The wound healed in less than a week, leaving a very small cicatrix.

6th August 1872.—Mrs N., æt. 23, married about six months. Patient has an unhealthy suspicious-looking sore, destroying the tragus and spreading down the cheek. It began about a week previous as a small pimple with a "watery head." She scratched it; since then it has gone on destroying the tragus. The surface of the sore is covered with a yellow slough, edges ragged. Patient states that five months previous she had a sore on her left breast under the nipple; cicatrix still visible. The sore on the tragus was touched with nitrate of silver, and dressed with a tepid solution of

Condy's fluid. Donovan's solution was given inwardly. Under this treatment, with the addition of syrup of the iodide of iron, the sore was covered with a healthy cicatrix in less than six weeks.

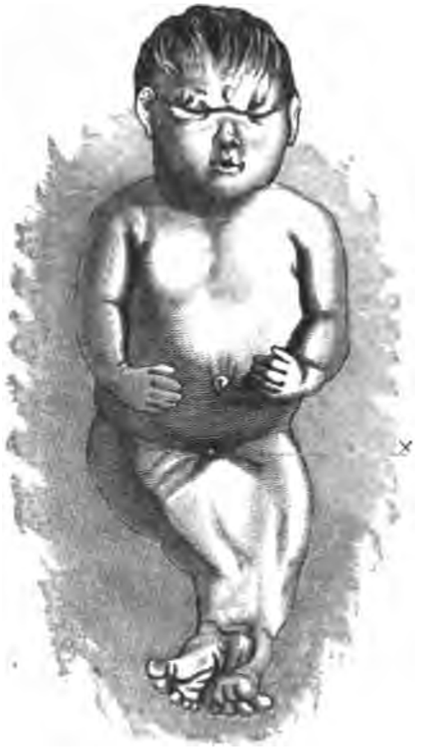
8th August 1872.—Miss B., æt. 27, had worn ear-rings since childhood. About six years ago she wore for some months black ear-rings, the wire loops of which were coloured black. This colouring matter came off; at last, when the ear-rings were removed, on account of a redness, swelling, and itchiness of the lobes, the wires were yellow instead of black. About that time, a swelling was noticed at the back of each lobe; ear-rings could still be worn. These swellings have gone on increasing, and for some months back have prevented the wearing of ear-rings. Washes, pressure, and other remedies have been tried for their removal or to stop their growth. At present each is as big as a large hazel-nut, very hard, and the skin firmly stretched over it. They can be seen pushing forward the lobe, and projecting below it, when looking in the patient's face. There is no opening to be seen in the skin over these growths, but the opening can be seen in front of the lobe, and a wire can be pushed in for about half an inch. They were both removed on the same day, an elliptical piece of skin being taken with each mass, where it was firmly adherent, as it did not appear to be required to cover the raw surface. There was considerable arterial bleeding, which in the case of the left lobe was touched with a point of lunar caustic, and firm pressure applied by compressing the lobe back upon the mastoid process. The compression of the lobe of the right ear upon the mastoid process sufficed. They were kept in this position for twenty-four hours, cold being applied over the bandages. The wounds in both were healed up in less than a week. Since then there has been no return of the growths. On making sections of the tumours, the piercing could be traced as a black point more than half-way through. Sections made transversely to the "piercing" showed concentric rings of fibrous structure, the blackness shading away from the black point in the centre.

ARTICLE V.—*Case of Siren-like Malformed Fœtus at Full Time, with a Well-formed Penis on the Posterior Aspect of the Body.* By WILLIAM MACLAREN, M.B., C.M. Edin., Lasswade.

EARLY on the morning of the 16th October I was called to attend Mrs B. in her ninth labour. On reaching the house I found that labour had been progressing steadily for some few hours previously, and on examination found the head in the vulva, and every prospect of the child being born in one or two pains, which was the case. On delivery the child was alive, but soon died. The placenta had undergone considerable fatty degeneration, and was very friable.

Considerable hæmorrhage followed the expulsion of the placenta, but was soon controlled, and the mother has done well.

Fig. 1.



REMARKS ON THE FŒTUS.

Measurements.

Occ. front,	13⅛ in.
Extreme length,	18 „
Girth of chest,	12¼ „
Weight,	4 lbs. 10 oz. 6 dr. imp.
Girth of pelvis,	10 in.

From the pelvis downwards both limbs are fused into one pyramidal mass, bifurcating into a fan-shaped arrangement of the feet, the soles pointing anteriorly, and surmounted by three toes on each side of the central line, one in the middle, and one about a line behind, in the dorsum of the mass. Anteriorly, at the junction of the abdomen with the lower extremities, two inches below the umbilicus and exactly in the mesial line, is a small papular nodule of skin, without any opening (Fig. 1, X).

At a corresponding point posteriorly, and 1½ inches below the coccyx, hangs, like a caudal appendage, a well-formed penis, with a pervious canal, without any trace of scrotum, testis, or anus (Fig. 2, P).

The nates may also be said to be absent.

The limbs are joined together in their inner and posterior aspects, so that both patellæ are found on the outer side quite movable.

The hip-joints move freely antero-posteriorly only.

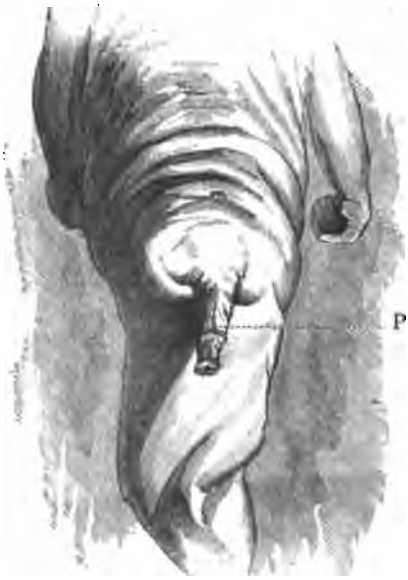
The knee-joints are indistinctly mobile, and the ankle-joints less so.

The nails on the fingers are well developed, fully so on the great toes, and becoming less distinct as they diminish in size.

The upper part of the body is, both in measurement and appearance, natural.

It is interesting to note that the eldest child of this woman, who lived for twelve years, had a patent foramen ovale.

Fig. 2.



The specimen has been sent to the Anatomical Museum of the University of Edinburgh.

ARTICLE VI.—*On Idiocy, especially in its Physical Aspects.* By Dr W. W. IRELAND, Larbert, Stirlingshire.

THE following article ought to have appeared shortly after the paper "On the Mental and Sensory Deficiencies of Idiots," in the last February number of the Edinburgh Medical Journal. In the present paper I shall confine my attention mainly to the physical and pathological aspects of idiocy. The causes of idiocy have been well treated in the little work of Dr S. G. Howe, of Massachusetts. It appears that whatever weakens the organism of the parent may be the predisposing cause of idiocy in the offspring. The scrofulous diathesis especially seems to favour its production. In other terms, there is a larger proportion of scrofulous or weakly parents who have idiotic children than of healthy parents who have idiotic children; but we cannot at present establish any closer connexion between scrofula, drunkenness, rachitis, and other manifestations of a weak and depraved constitution on the one hand, and idiocy on the other, or explain why these unhealthy states should in some cases check the growth or nutrition of the nervous centres, instead of affecting the growth or nutrition of other parts of the body or of all the organs alike.

Scrofula is certainly very common with idiots; but then again we know that many scrofulous children descended from scrofulous parents are of average intelligence, and that some have unusual mental activity. In cretinism we have a right to assume the existence of a specific cause existing in certain localities principally shut in valleys. This cause, whether existing in the air or water or both of them, is only known to us through its effects; but its action is much increased by the operation of whatever tends to weaken the constitution of the child in the district where cretinism is endemic.

Thus, children brought up in confined, dark, and dirty houses, ill fed and ill cared for, are more liable to become cretins than children who are brought up in the same place, but under more happy surroundings. Still, we see children brought up under every condition favourable to health sometimes falling a prey to the cretinsm in places where the malady is rife. In a similar manner, although the children of parents whose constitution is weak are probably more liable to have idiotic children, it not unfrequently happens that idiots are born into a family where the father and mother, and the sisters and brothers, are apparently quite healthy, and live in obedience to the laws of health. There is, however, no doubt that in families where neuroses exist, as manifested by lunacy, imbecility, epilepsy, deafness, or other like disorders, idiocy is much more frequently met with.

It has been advanced that idiocy is a proof of the dying-out and degeneracy of a family, but it would seem from the statistics of the

Larbert Institution that the number of children in families where idiocy has appeared is not less than the average. Out of 145 families, the average number of the children was 5·11, and although those who were idiots must be regarded as dead branches, many of the other brothers and sisters were vigorous and intelligent. Out of 149 families, seven had married their first cousins, and of these cases two had each two children imbecile, and one had three such children. The marriage of blood-relations seems to strengthen a pre-existing tendency to idiocy or insanity in the offspring, but does not appear to call it into existence. In places where cretinism is endemic, frequent intermarriages have been found to increase the proportion of cretins amongst the children.

In a healthy race living a pure and healthy life, I imagine that idiots will be rare, and that the proportion of those who become so from accidental causes will be greater; but in a society struggling under unhealthy and disquieting influences, idiotic children will be more numerous, especially if these influences act upon the constitution of the mother. Dr Seguin, in a lecture[1] delivered before the New York Medical Association, remarks, that idiocy is increasing in the State of New York, and finds the cause of it in the unsatisfactory social conditions which some people wish to introduce into this country.

"We overburden women," says Dr Seguin, "they overburden themselves, and choose or accept burdens unfit for them." "As soon," he says in another place, "as women assumed the anxieties pertaining to both sexes, they gave birth to children whose like had hardly been met with thirty years ago—insane before their brain could have been deranged by their own exertions—insane, likely, by a reflex action of the nervous exhaustion of their mother."

The antagonism created by social agitators of the gynagogue class seems to Dr Seguin to have already borne evil fruit. "Children gotten under such moral and other pressures cannot truly be said to be born from the union, but rather from the disunion, of their parents; conceived in antagonism, they can only be excessive in their tendencies, or monsters in their organization."

The unquiet, speculating life led by many in New York, where riches and extravagance, disappointment and failure, swiftly follow one another, has effects which Dr Seguin points out in a striking instance. "I have attended a mother of a remarkably fine family of four children, whose fifth was affected in this wise. During that pregnancy, her husband was deeply involved in speculations; he would say nothing then about his chances, but she knew daily, by the way he ate, how much he had lost. One day she saw him swallow his dinner without masticating at all; she fainted away, the child hardly moved after, and was born a cripple and an idiot."

Fright to the mother during pregnancy is a not very uncommon cause of idiocy.

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[1] New Facts and Remarks concerning Idiocy, by Edward Seguin, M.D. York, 1870

Besides these general causes, whose vagueness is not favourable to scientific inquiry, there are more determinate exciting causes; the child becomes idiotic either through lack of development or nutrition, or through disease or injury befalling the brain before or after birth.

Certain diseases bring idiocy in their train, and much may be learned from studying the conditions under which this takes place. Some authors have divided idiocy into congenital and acquired,—a distinction which appears to me of little value where the pathological causes are the same. Most of the diseases which produce idiocy may occur before birth as well as after it, and we do not require a double pathological classification without any sufficient difference either in the nature of the lesion or its results. This brings me to the classification of idiocy, which I attempted, based upon pathological conditions. If we define idiocy as simply a deficiency of the psyche, then a mental qualification is what we must logically aim at. If, on the other hand, we define idiocy to be mental deficiency depending upon mal-nutrition or disease of the nervous centres occurring either before birth or before the evolution of the mental faculties in childhood, we look at it in a different aspect, and must classify it in a different manner. In studying idiocy in connexion with the diseases which give rise to it, one feels the want of some such classification, nor can we be content with a mental classification without a pathological one, or a pathological without a mental one. The human mind, bounded in its insight, requires to look at the subject in two aspects, and for the same reason that we require to see a solid body on every side, unless it happens to be transparent. In a similar way, we have different classifications for human beings according to the point of view from which we regard them. Take for example Socrates and Cæsar, Confucius and Timur Khan. In respect of their mental powers and pursuits, Confucius and Socrates would fall into the same rank as philosophers, and Cæsar and Timur Khan would come together as rulers or warriors. But then we might classify them with the eye of a zoologist, when Cæsar and Socrates would go together as Aryans, and Confucius and Timur Khan as Mongolians. In a like way, an epileptic idiot might fall under the same mental division as a hydrocephalic one, if we classify him from the degree of intelligence left. Nevertheless, we cannot refuse to take into consideration the modifying influence of the special disease upon the course and prognosis of the idiocy; and as a general rule the different divisions of idiocy proposed below have certain mental characteristics in common, somewhat in the same way as the Mongolians and Aryans seem to have got a prevailing character. It is clear to me, whatever may be the fate of the whole classification, that some of its divisions, as cretinism, epileptic and paralytic idiocy, have such marked characteristics that they will never be given up.

Coming to the study of idiocy after having gained some experience in medicine, I have from the beginning viewed it from the standpoint of pathology; and my idea of idiocy is compounded of the following classes, which are generalized from individual existing idiots who resemble one another by having the same or similar diseases, as they resemble the type of idiocy by having mental deficiency with a corporeal disease.

1. Hydrocephalic Idiocy. 2. Eclampsic Idiocy. 3. Epileptic Idiocy. 4. Paralytic Idiocy. 5. Inflammatory Idiocy. 6. Traumatic Idiocy. 7. Microcephalic Idiocy. 8. Congenital Idiocy. 9. Cretinism. 10. Idiocy by deprivation.

Of these forms, to which more may yet be added, I shall now treat in their order.

Hydrocephalic Idiocy.

Hydrocephalus is one of the most fatal of the nervous diseases of children, as may be seen by a glance at the Registrar-General's Returns; but while many children die of water in the head, and some recover without their intellects being impaired, there are a few who neither die nor recover—they become idiotic. The disease turns chronic; the pressure of the fluid relaxes; the head ceases to increase; Wormian bones are thrown out where the edges of sutures are distant from one another; and the brain to a certain extent recovers from the pressure and stretching which it has received. Sometimes the hydrocephalus is congenital, coming on before, or more often shortly after, birth; at other times the hydrocephalus is acquired, coming on in the first few years of childhood. In this case it seldom appears before the tenth month, and is commonest from the fourth to the seventh year.

In some cases the hydrocephalus seems to have a dwarfing effect upon the general growth of the body; sometimes the child is deaf, probably from the stretching of the portio mollis within the skull through the effused fluid. Occasionally hearing is restored owing to the tension being relaxed or to the nerve becoming accustomed to it; more rarely blindness follows water in the head,[1] perhaps through the pressure of fluid upon the corpora quadrigemina, or on the optic nerves at the tuber cinereum. The sense of touch is in some cases dull. Squinting is not uncommon. The voice sometimes undergoes a peculiar alteration; the patient speaks in a lower key, and with a hollow timbre.

From the peculiar form of the head, there is generally little difficulty in recognising hydrocephalic idiots, though it occasionally happens that the head is not larger than usual, where the brain has been much diminished by the pressure of watery effusion; and some men with heads hydrocephalic both in size and shape are possessed of ordinary intelligence. The size of the head does not form a safe criterion as to the amount of injury done to the mental powers. Seguin, in his book on Idiocy,[1] gives the case of a girl seventeen years old, whose head was thirty-seven inches in the largest circumference at its summit, and twenty and a half inches over the vertex from one foramen auditorium to another. The hydrocephalus commenced in the first year of life. "She began to speak when five years old. Her senses have always been acute, but she lost her sight five years ago. In 1865, she had spasms for the first time; since, she has grown weaker, and entered already in her period of decay. Though her brain was, we may say, drowned in an ocean of heterogeneous fluid, it kept up active communication with the world. She was cheerful, sung and talked until lately, used playthings as ordinary children do, liked to see bright objects waving before her eyes when she could see, and even now in cæcity she amuses herself by making papers rustle and move before her absent vision, muttering yet, though indistinctly, 'Hurrah for the colours,' 'Hurrah for the flag;' touching reminiscences of popular festivities which impressed at an earlier period."

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[1] Dr Rud Leubuscher, in his work "Die Krankheiten des Nervensystems," Leipzig, 1860, s. 221, says that injury to the sense of hearing is rarer than loss or diminution of the sense of sight. My own remarks, however, refer to hydrocephalic idiocy, not to acute or uncomplicated hydrocephalus.

As Rokitansky has observed, we ought to distinguish between acute or chronic hydrocephalus and hydrocephalus *ex vacuo*, in which fluid is poured out simply to fill up a reduction of the volume of the brain. This, however, is much more common in old age than in childhood. Hydrocephalus has been distinguished into internal and external. In the one, the fluid distends the lateral ventricles; in the other, it is between the brain and the membranes. Hydrocephalus externus alone is not very common; but there are some remarkable cases on record, where a large amount of fluid was collected between the skull and the brain, the brain being placed at the base of the cranium. In these cases the intelligence does not appear to suffer so much. It is not unfrequent to have hydrocephalus externus and internus at once. As Cruveilhier has remarked, the cerebellum generally escapes from the pressure of the effused fluid. Hydrocephalus is not unfrequently combined with epileptic fits, and sometimes with paralysis; and in these cases the prognosis is not so good.

Hydrocephalic idiots are frequently of very feeble constitution, and of a tubercular or scrofulous diathesis; nevertheless, if in tolerable health, they are more educable than some other classes of idiots, and generally improve under training. They are, as a rule, gentle in their disposition, and somewhat awkward in their motions.

As hydrocephalus leads to obtuseness and dementia rather than excitement and mania, it is not commonly seen in the brains examined in lunatic asylums.

Dr Leubuscher[2] has recorded some remarkable cases where consciousness has reawakened shortly before death, which he explains by the supposition of a partial absorption of the effused fluid. I have an account of a girl believed to be idiotic from hydrocephalus, who was noticed to improve mentally during her last illness, which was apparently consumption.

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[1] New York, 1866, p. 340.

[2] Op. cit., p. 22.

Eclampsic Idiocy.

Though eclampsia may appear in children of healthy parents, there is no doubt a predisposition hereditary in some families to take fits under exciting causes, which cannot be avoided. The most general, as well as one of the earliest of these exciting causes, is the cutting of the teeth. The child is thrown into fits, often long continued and returning with short intermissions, placing its life in the utmost danger. Most of those who survive, escape without any apparent injury; but some become permanently idiotic. Further experience has only confirmed the unfavourable prognosis which I ventured to lay down of eclampsic idiots, in my article upon the "Classification and Prognosis of Idiocy," in the *Journal of Mental Science.*[1] "Though the power of muscular motion, as well as the tactile sensibility, is generally well preserved, and special sense does not appear to be injured, the intelligence is in a great degree destroyed, and the child remains, comparatively speaking, uneducable. He can be taught more readily to work than to think." Often the child remains a mute. To this unfavourable prognosis there are exceptions, but relatively not very numerous. The remark of a writer on Epileptic Insanity[2] seems to hold good with our cases. "No principle has received a greater sanction from experience than that the earlier the age at which epilepsy springs up, the deeper it undermines the organic and moral constitution, and the more disastrous are its results."

The difference between eclampsic and epileptic idiocy is, that in the first form there is a succession of fits following close upon one another; after a time the child recovers its bodily health, the fits cease, leaving behind alterations in the structure of the brain, which have disordered and arrested its health and nutrition. In epileptic idiocy, on the other hand, the convulsions recur at more distant intervals; but the disease does not show the same tendency to disappear. A total cessation of the fits is rare, and the physician is generally pleased if he can succeed in increasing the length of the intervals between each attack by medical or dietetic treatment.

Epileptic Idiocy.

I apply this name to those cases where the epilepsy seems to be the cause of the mental obtuseness; for it ought to be kept in mind that congenital idiots are now and then subject to epileptic fits, which need not necessarily have a marked effect upon the intelligence, and in any case can only be regarded as a complication. Epilepsy, as is well known, is one of the commonest causes of insanity, as well as of idiocy, and in making our definition of classes it is difficult to know where to draw the line between epileptic idiocy and epileptic dementia. It is inconvenient to draw the line so as to include children under the heading of lunatics, in case they should be sent to asylums for the insane, where there is no proper provision for their training and treatment, and where, owing to their proneness to imitate bad examples, they rapidly deteriorate. If the epilepsy has caused the faculties to become impaired before the age of seven, it appears to me that the patients ought to be treated as epileptic idiots. There is a strong feeling against the admission of epileptics into training schools for idiots. They are often wild and irritable, and, though they generally possess an amount of intelligence which deceives those who do not take into consideration the existence of a disease which may be expected again and again to return, they are not easily taught, and are liable to lose what they have learned. If some progress has been made through the unwearied efforts of the teacher, a renewed attack or series of attacks has a tendency to wipe the new accomplishment away, and the teacher has to recommence his work over again. However, I am convinced that this unfortunate event is not so common as represented by some writers. With the majority of our epileptic pupils the continuance of the fits no doubt retards the growth of the intelligence, and the pupils are duller and heavier for a few days after an attack. When this has passed away, they again commence their tasks and take up their interrupted lessons without any loss of acquired knowledge being noted. In one case at least, in the Larbert Institution, the patient is generally duller before the fits come on, and his intelligence does not appear to suffer much from the immediate effect of the attacks, which are sometimes severe. On the other hand, the probability of a complete cure of idiocy is greater in the epileptic idiocy than in any of the other classes, save that of idiocy of deprivation.

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[1] October 1872.

[2] Dr M. G. Echeverria, in the American Journal of Insanity, July 1873.

Dr W. A. F. Browne,[1] and Dr Isaac Kerlin of the Pennsylvania Training School, have recorded cases of cure of epileptic idiocy, and take a more hopeful view of their prospects, and advocate their claims for admission to charitable establishments, from which they have been excluded. I have given a short abstract of a remarkable case,[2] which it may be worth while to reproduce.

A girl, whose father was a drunkard and epileptic, had fits when three years old, from which she recovered, and grew up to the age of fourteen without any deficiency of intelligence or bodily disease. "About this age the epileptic attacks again returned, and became very frequent, when she passed by degrees into a fatuous state. Her gait became tottering; often she was unable to sit up even in an easy chair, and she even required to be fed. Sometimes she would wander about at night, instead of sleeping, or fall into maniacal bursts of passion. After four years' illness, she was admitted into the asylum under Dr Wiedemeister, of Osnabrück, where she passed her time in the compartments used for dirty and noisy patients. The fits were both severe and frequent, and she was treated with bromide of potassium and oxide of zinc. Little hope was held out of her recovery; but, after being a year in the asylum, and five years ill with epilepsy, the fits began to be less frequent. In November, her catamenia first appeared, and in December the last fit was seen. She began to do simple pieces of work, her strength revived, and her intelligence gradually returned, and on the 23d of June 1872, after being three years in the asylum, she was dismissed, cured of imbecility and epilepsy. On her returning to the world, it was found that she had forgotten many things which she had once learned. She had again to be taught to sew and knit. She had entirely lost the use of speech; even during the period of her deepest fatuity, she used to sing songs which she had learned in the school, generally with the proper tunes; and when she was again sent to her lessons, it was found out she had not entirely forgotten reading and writing. During her fatuous condition she was not able to recognise her mother, and on her recovery she had entirely forgotten that her mother had pigs and cows; nor did she recognise her native place, nor even her home, when brought back to it."

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[1] See Dr Browne's views in his lecture on "Epileptic Mania," Journal of Mental Science, vol. xi. p. 352.

[2] In the German Retrospect of the Journal of Mental Science, July 1873.

Schroeder van der Kolk[1] gives another instance of a lad of seventeen, who was nearly idiotic from frequently-recurring attacks of epilepsy, who recovered after incisions being made in the scalp, and issues being kept up. "His mental powers, although not greatly developed, were remarkably improved; writing, arithmetic, etc., progressed satisfactorily; while he had a situation in a factory. The only peculiarity he manifested was tolerably constant irritability of temper. It was, however, extremely important that from time to time, instead of an attack, hæmorrhage took place from the wound, with the effect of greatly clearing his head."

The lad remained for eighteen months free from epileptic fits, when he died of consumption. "From this case," observes the distinguished Dutch pathologist, "it is particularly clear that idiocy, or apparent dementia, after epilepsy, is quite a different disease from dementia after idiopathic mania, which always depends upon degeneration and atrophy of the cortical substance, and is incapable of cure."

Cases like these should be held in mind to save us from desponding. Unfortunately they are not common, and Dr Shuttleworth, once Assistant-Physician at Earlswood, and now Medical Superintendent of the Royal Albert Asylum at Lancaster, has remarked that there were eighty epileptics at Earlswood, and that of these there were between forty and fifty cases incapable of improvement, whereas of the other idiots no more than six per cent. were entirely unimprovable.

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[1] On the Spinal Cord and Medulla Oblongata, and on Epilepsy. Sydenham Society, London, 1859, p. 272.

The rules of the Larbert Institution are not favourable to the admission of epileptic idiots, and the less promising cases are generally excluded; but of eighteen patients who found their way into the house, seven did not appear to improve, one of them is not recorded in the case-book, four improved a little, and six improved considerably. In three of these cases especially the progress was very satisfactory. The pupils increased in intelligence, and the fits diminished so notably in number, that I consider a complete cure not improbable. Of two of these cases, I had at one time a very unfavourable opinion, and I know that it was at one time contemplated to remove them from the house. In the matter of treatment, bromide of potassium seems to have a less favourable effect upon the inveterate cases of epilepsy which come into an institution for idiots, or into an asylum for lunatics, than it has in outdoor practice. Where improvement follows treatment, I am disposed to attribute it as much to diet as to medicine. Benefit is sometimes derived from excluding flesh meat from the dietary of epileptics, a piece of advice which I got long ago from Trousseau, on his visits to the Hôtel Dieu, and which has been lately repeated by Dr Hughlings Jackson. It is likely that the gymnastic training given by us is also of advantage in treating epilepsy.

"During the last three or four years," remarks Dr Radcliffe, "I have seen several cases of epilepsy, chorea, and hysteria, in which undoubted good has resulted from the adoption of a regular course of suitable gymnastic exercises."[1]

However great the improvement may be, the patient is never secure against falling back again, save by the total cessation of the epileptic fits. Sometimes the mental powers continue to increase until puberty or later, and then the fits become more frequent, and either bring the sufferer to his end or render him insane. In some patients met with in ordinary practice, epileptic fits seem to occur for many years without apparently diminishing the mental power, which, however, in these cases is seldom of a brilliant character.

Such is our dearth of direct observations that we are willing to allow Dr J. Crichton Browne's[2] opinion to pass for the time that the lesions in the epileptic idiot are the same as those which are described in the epileptic dement, and it is not unlikely that there may be little difference between the morbid appearances in the eclampsic and epileptic idiot. The morbid changes which he has found after epileptic dementia are atrophy of the brain, evidenced by some opacity of the arachnoid, diminished size of the gyri, and enlargement of the sulci, which contain some compensatory serous fluid, a quantity of which also generally occupies the enlarged ventricles. The atrophy of epilepsy is moderate in degree. It rarely approaches that of old age, chronic alcoholism, or simple brain-wasting.

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[1] Lectures on Epilepsy, Pain, Paralysis, etc., by C. B. Radcliffe, M.D. London, 1864, p. 227.

[2] Journal of Mental Science, April 1873.

Out of fourteen examinations of the brains of patients suffering under dementia and epilepsy, MM. Bouchet and Kazauvielh found hyperæmia of the brain and membranes in fourteen cases, indurations partial or general in seven cases, softening partial or general in seven cases, and the convolutions small and atrophied in seven cases.[1] Atheroma of the vessels is not commonly met with; but traces of extravasated blood are sometimes observed under the membranes. Dr Howden, of the Montrose Asylum,[2] observes that "effusions in the pia mater are not unfrequent in epileptics; those I have met with are of old standing, and had assumed the character of rusty gelatinous deposits." Trousseau[3] believed that after severe attacks of epilepsy slight effusions of blood are produced in the substance of the brain, in the membranes, and in the spinal cord, similar to the ecchymoses which appear in the subcutaneous tissue of the skin. Effusions of larger size have been described by Calmeil[4] and others. These effusions explain the paralysis which sometimes follows epileptic fits, and which may pass away in a few days. On the other hand, Dr Russell Reynolds[5] remarks, that although the epileptic convulsions are not rarely followed by profound coma and a general appearance of apoplexy, "it is exceedingly rare to find that actual cerebral hæmorrhage has occurred."

It is likely enough that in epileptic attacks which only obscure the intellect in a passing degree, dilatation of the vessels is the only lesion produced, and that this is succeeded by albuminous deposit outside the capillaries when the attacks become more severe and persistent, and that hæmorrhage only appears in those violent cases which are found in lunatic asylums. In several instances where I had an opportunity of being present at the post-mortem examination of epileptic lunatics, increased hardness of the brain and dilatation of the vessels were noted. Hardening of the olivary bodies was not always present; but in some cases was very well marked. In other instances, no change visible to the naked eye was discoverable.

These alterations, noticed by different writers, are the results of the epilepsy, but in their turn the causes of the idiocy or dementia.

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[1] Récherches sur l'Encephale, sa Structure, et ses Maladies, par M. Parchappe. Paris, 1838, p. 200.

[2] See his Analysis of the Post-mortem Appearances in 235 insane persons, p. 5.

[3] Clinique Médicale. Paris, 1868, tome ii. p. 97.

[4] There is a most instructive series of cases of brain disease, with their accompanying lesions, in the Traité des Maladies Inflammatoires du Cerveau, par le Dr L. F. Calmeil. Paris, 1859.

[5] Reynolds on Epilepsy, quoted by Dr Howden, op. cit.

Paralytic Idiocy.

Cerebral apoplexy is not common with children; but it is clear that paralysis associated with idiocy must have a centric origin. I have seen about a dozen of such cases. They seem to improve mentally rather than physically. The mental powers could be increased by cultivation; but the paralyzed limb either did not acquire strength, or improved by slow degrees.

In the *Journal of Mental Science* for October 1873, I have given the description and necroscopy of a case of paralytic idiocy which began in the eighth month of the child's life. He died when eight years old of bronchitis. No trace of apoplectic clot could be found in the brain. There was a spot of white softening about the size of a walnut on the roof of the left ventricle above the posterior cornu. This lesion, whatever its date, was on the same side as the paralysis. The lateral ventricles were distended with fluid. The brain was fully formed.

Injuries to the brain causing paralysis may take place before birth as well as after it. Before birth the brain may suffer lesions which would entail loss of life after birth. Hence the cases of paralytic idiocy where there is most destruction to the nervous tissue are congenital ones.

Schroeder van der Kolk has an interesting case of atrophy of the left hemisphere of the brain occurring in a female imbecile, aged 27. She had from her earliest infancy been paralyzed on the right side. The right hemisphere of the brain was quite healthy, but the left was much smaller. The difference of size extended to the base of the brain. The cerebellum was atrophied on the opposite side, that is, the right side, and below the decussation; the atrophy of the spinal cord passed also to the right side from the left hemisphere. The learned author has collected a considerable number of such cases where there was paralysis and diminution of one side of the body, and the opposite side of the brain was atrophied. This appears to be owing to inflammation either before birth or shortly after it. It is to be noted that in these cases when one hemisphere was completely destroyed, the paralysis is not total. The arm is generally more paralyzed than the leg.

"With respect to the symptoms produced by this unilateral atrophy of the brain, they manifest themselves partly in the more or less defective exercise of the mental powers, and partly extend their influence, as I have shown at length in the above case, over the rest of the body. That in atrophy of one-half of the cerebrum, the psychical powers should be blunted or paralyzed, might perhaps be assumed as generally true (and, in fact, such atrophy is most usually met with in idiots); still it is far from being universally the case, for although in some instances mention is made of rather blunted mental powers, examples also occur where, with atrophy of one hemisphere, the intellectual faculties appear to be in their normal condition."

He refers to a number of curious cases going to show that the loss of one-half of the brain may be combined with the perfect use of the intellectual faculties.[1]

"Everything, in my opinion, depends upon the more or less healthy state of one hemisphere of the brain. If, as from the nature of the case seldom occurs, the inflammation and affection of the pia mater has not extended to this hemisphere, if the gray matter under the cerebral convolutions has here continued perfectly sound, there is no reason why this remaining hemisphere should not be able to act without impediment in the exercise of those functions which are necessary to our mental powers, just as one eye sees as sharply though the other be lost. But where the gray matter is injured in both hemispheres, particularly anteriorly, disturbance of the intellectual faculties will be inevitable."

Dr Schroeder van der Kolk adds in a note:—"A remarkable case, where there was probably atrophy of one-half of the brain, is communicated by Wigan, in Forbes Winslow's *Journal of Psychological Medicine.* (See Damerow's *Algem. Zeitschrift für Psychiatrie.* Berlin, 1851, 8 b. 2 h. p. 279.) A boy, aged fifteen, had an inequality of the skull, as if the left half of the brain was cut off from above towards the ear, and was covered with a flat bone, so that the size of the left hemisphere could certainly not amount to more than one-third of that of the right; at the same time, he had a tottering gait, without being paralyzed. This boy, who at first appeared to be quite idiotic and incapable of learning or understanding anything, with a stupid appearance, had, under constant instruction, three years later, grown into a strong lad, and his intellectual faculties were fully developed, while his brain was increasing in size, the left hemisphere, however, always continuing about one-third less than the right."

In another note he says:—"Since the publication of this essay, I have six or seven times observed in living subjects this lesion with shortening and atrophy, especially of the arm, opposite to the atrophy of the hemisphere. In the majority of cases, a greater or less degree of idiocy existed; perfectly unimpaired intellectual powers I never witnessed with this lesion."

In these cases there is often no inequality in the size of the skull on the atrophied side; the calvarium is either thickened, or the empty cavity is filled up with fluid.

It does not seem to me necessary to make any distinction between those cases where the paralysis has occurred before birth or in early infancy. Indeed, it is sometimes difficult to ascertain whether the paralysis was congenital or not. I have had good opportunities of studying one case, where the paralysis must have begun before birth. The head is of average size, and appears symmetrical. There is no deficiency of the senses on either side, but the left side of the body is everywhere smaller than the right. The left arm is more paralyzed than the leg, and shorter and much thinner than the sound one. The hand is bent upon the forearm, so as to make it quite useless. The left leg is thinner and shorter than the right, so that he limps, and cannot walk far without exhaustion. This boy, who is now thirteen years old, though decidedly imbecile, has a good deal of shrewdness and some humour; but is slower at the school than one would suppose from his conversation. He can read words of one syllable, and is good at music, but deficient in arithmetic.

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[1] Translated from the original, by Wm. Daniel Moore, M.D., T.C.D., etc. New Sydenham Society, London, 1861, pp. 150, 154, 155.

It not unfrequently happens that idiots cannot pronounce particular sounds or letters, or can pronounce them only in particular combinations. They often substitute one letter for another. One imbecile female, aged twenty-three years, cannot sound P, T, and K, using in their places, B, D, and G, the sounds which require an adjustment of the muscles of voice most nearly resembling the letters she cannot imitate; B and P being formed by approaching the lips, D and T being formed by approaching the tongue to the arch of the palate; G and K are gutturals. In B, D, and G, the mouth is closed and opened more slowly than in P, T, and K, in which the action of the lips, tongue, and throat is more abrupt. This woman has the upper alveolar ridge so prominent that the lip does not stretch properly over it. It is often a difficult question whether these deficiencies in pronouncing certain sounds are owing to paralysis of different nervous filaments or motor centres of nerves, the remains, perhaps, of more extensive paralysis or nervous weakness. In some instances this is no doubt the true explanation. We have, for example, a boy aged thirteen, partially hemiplegic, who was utterly unable to pronounce K at the beginning of a word, and sounded the letter G imperfectly. This was owing to deficient power in the muscles of the uvula and soft palate. On being asked to pronounce K with the mouth open, the uvula could be seen to be drawn to the side opposite to that on which the arm and leg were paralyzed.

We have a child ten years of age who only began to speak about two years ago. At present, though possessing more intelligence than many children who talk volubly, he cannot pronounce many sounds, such as K, Th, B. He always substitutes T for K, and P for B. This boy has a slight paralysis of the left side of the face. We have many other cases of children who cannot pronounce particular sounds, in whom no other muscular deficiency appears to exist. There are, however, other cases in the house, who have special deficiencies of motion in the arms or legs, which appear at gymnastic drill.

One difficulty in regarding these as cases of deficient pronunciation appears to me that most of the muscles used in speech are the same as those used in chewing and swallowing the food; and if we assume paralysis in the one function, how do we account for it not taking place in the other? We know that in labio-glosso-pharyngeal paralysis, which has generally been found to be associated with disease of the pons, or of the corpora olivaria, swallowing is impaired along with that of speaking. This consideration appears to me not without weight; but it ought to be borne in mind that the articulation of words demands a much finer adjustment of the muscles than in moving the lips or swallowing, and that a loss of power over the muscles generally commences with difficulty or hesitation of the speech before any other motions are affected. This is well known to be the case in drunkenness and general paralysis of the insane. Stammering consists in the momentary inability to pronounce a sound or to connect it with other sounds, and is owing to deficiency in the co-operative power of the larynx with the movements of the mouth; but though stammering may be associated with chorea of other muscles, it often exists with perfect command of the rest of the muscular system.

Max Müller remarks:[1] "There is one class of phonetic changes which take place in one and the same language, or in dialects of one family of speech, and which are neither more nor less than the result of *laziness*. Every letter requires more or less of muscular exertion. There is a manly, sharp, and definite articulation, and there is an effeminate, vague, and indistinct utterance. The one requires a will, the other is a mere *laissez-aller*. The principal cause of phonetic degeneracy in language is when people shrink from the effort of articulating each consonant and vowel; when they attempt to economize their breath and their muscular energy."

This species of indolence is very common with idiots. They alter difficult sounds for others which they can pronounce more easily, miss out articles, and shorten words; the tendency being to reduce all words to one syllable.

A singular analogy may be found between the aberrations of idiots from common speech, and those which time has brought about amongst peoples who once spoke a common dialect, as in the Aryan and Semitic families, each from a separate *Ursprache*, and the better-known example of the derivation or corruption of the different Romance languages from the Latin. It appears, as Max Müller argues, that in the end we must come to a physical explanation in the structure of the organs of speech. Certain races prefer certain sounds, because they can utter them more easily; and this must be referable to a facultative difference of nervous or muscular energy giving greater or less power to particular muscles, so that some modifications of the expelled air of the chest are more easily made than others by the majority, and consequently such-and-such a pronunciation is preferred. In this way whole nations have given up using sounds common in the language of others, just as idiots avoid or cannot pronounce certain sounds.

The Latin *aqua* became *agua* in Spanish, *eau* in French, and *apa* in Roumanian. From the same root the Romans formed *sto*, the

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[1] Lectures on the Science of Language, by Max Müller, M.A., etc. London, 1864, p. 176.

Greeks *ἵστημι*. Many nations find it difficult to begin a word without a vowel, and so the Welsh changed *schola* into *ysgol*, the French into *école*; the Spaniards in Peru said *escola*. The Hindustanis always say *ischool*, *istable*, when they have to use such words. The Chinese have no R in their language; the Mexicans had no B, V, F; the six nations amongst the American Indians have no labials.[1] The Arabians have sounds which we can scarcely imitate in their *khe* and *ghain*. The people of the Society Islands, in trying to pronounce Captain Cook's name, said *Tute*, exactly what several of our children would say. In like manner, I could find counterparts to many of these linguistic variations in cases of idiots, who in speaking interchange certain letters or sounds, but it would probably be tiresome to pursue the subject.

Inflammatory Idiocy.

I have not been able to collect many cases of inflammatory idiocy. It would appear from the researches of Jastrowitz[2] and others, that inflammation of the brain occasionally occurs before birth, and that this sometimes extends down the spinal cord. Such cases are at present not capable of detection until after death. It might be supposed that inflammation of the brain following upon scarlet fever, measles, typhus, and smallpox, would be common causes of idiocy, as they are of deafness; and they are frequently stated to be so by the parents of idiots. Nevertheless, it is extremely difficult for me, who rarely see the commencement of a case, to prove that idiocy, after all, did not precede, instead of follow, the fever. A physician in extensive practice in a large town has opportunities of studying the causation of idiocy from actual observation, which any one who has attended to the subject must know has a different value from the theories of parents.

The case of F. Q., published in the *Journal of Mental Science*,[3] seems to be one of inflammatory idiocy, and that of K. I. is probably another; nevertheless, it was not till after the necroscopy that this could be made out.

In a Report of the Institution at the Hague for Young Idiots,[4] out of 84 boys and 56 girls, two boys and one girl are said to have become idiotic from measles, one girl from typhus, and one from hooping-cough. The cases of inflammatory idiocy which we have met with were of different grades of intelligence; and it is evident that the injury from inflammation must depend in a great measure from its extent or situation, which we have no direct way of measuring during life.

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[1] Max Muller, op. cit., p. 163.

[2] See his valuable article on the "Encephalitis and Myelitis of Infancy," in the Archiv für Psychiatrie, Band iii. Heft i.

[3] October 1873.

[4] Vifde Verslag van het geneskundig Gesticht en de daaraan verbonden Dagschool voor minderjarige Idioten te 'Sgravenhage over de Jaren, 1864, 1865, en 1866, p. 72.

Traumatic Idiocy.

Traumatic idiocy ought to be distinguished from inflammatory idiocy, for although inflammation is likely to follow a blow on the head, it may be small in comparison to the damage done by the direct injury which the brain experiences from contusion, incision, division of the nervous tissue, or depression of the skull.

Owing to the softness of the bones of the skull in the new-born infant, as well as the looseness of the sutures, displacements and other injuries are not uncommon during parturition. As the head of the male infant is a little larger than that of the female, he is thus more liable to suffer injuries at birth. This may serve to explain why male children are more subject to idiocy, deafness, and diseases of the nervous system, than female children.

Dr Langdon Down,[1] in a paper on "Some of the Causes of Idiocy and Imbecility," has remarked, that "out of 2000, 24 per cent. were first-born children. He regarded two causes as potential in these cases: (1) pressure on the cranium, (2) suspended animation from retarded labour. He also attributed something to the more exalted emotional life of women during their first pregnancies. Among primiparous idiots, one-fourth had been born with suspended animation. While the ratio of sex among idiot primiparæ was 3 males to 1 female, the ratio of those born with suspended animation was 5 males to 1 female, indicating the influence of the increased size of the male cranium over that of the female."

I am not disposed to disagree with this conclusion of Dr Langdon Down, but cannot see how he strengthens his case by finding out that 24 per cent. of idiots were first-born children. Why not? Twenty-four per cent. of all children in England must be first-born children, and first-born idiots scarcely bear their due proportion to the healthy children of the country, for the average number of children to a marriage in England is 3·9; and if we remember that first-born are more numerous than other children, as sometimes there are no second-born ones. Dr Down's statistics seem here to be against his conclusion. Out of a hundred and forty-five families, mostly from Scotland, in which the average number of the children was 5·11, only 18·6 per cent. were eldest children. From this calculation four were excluded, as they were only children. The proportion of first-born children amongst idiots ought to be greater in England than in Scotland, because Scotch marriages are more productive (4·4 to 3·9); and the proportion will be higher still in France, where the average of children is still smaller than in England.

Though I am disposed to believe that the increased pressure to which the human head is exposed during labour is the cause of the preponderance of male over female idiots, there are cases which make me suspect a more occult influence. I know of instances where there were as many as three male idiots in a family, all the girls escaping. If there were a history of difficult labour, or suspended animation, occurring with the male children, and not with the female, the causation would of course be clear; but I have reasons for believing this cannot be made out. It is worthy of notice that, in a list of thirty-nine microcephales furnished by Vogt, only ten are females.

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[1] See the Report of it in the British Medical Journal, 11th Oct. 1873.

As elsewhere observed, the older a child gets the more liable are injuries to the brain to prove fatal; hence the number of cases where idiocy can be traced to falls or blows upon the head is not very large. At the same time, parents are extremely ready to attribute the idiocy of their children to an accidental fall or blow. As such accidents, however, do not often occur till the child begins to walk, and as this is almost always late in the case of idiots, we have here a criterion which ought to be made use of. In any case of idiocy which is put down by the parents to a fall on the head, the medical practitioner ought to make careful inquiries as to the existence of hereditary neurosis, the occurrence of fits or other diseases likely to be the cause of idiocy, as well as the nature, extent, and immediate consequences of the injury in question. There are, however, cases of idiocy which must be put down to external violence to the head. Setting aside those occurring at birth, such cases generally belong to the higher grades of imbecility, and, as the reader may imagine, they are not easily classified.

The amount of injury to the mental powers is variable, depending upon a number of circumstances it would be difficult and tedious to specify. In most of the cases which I have seen, the patient was simple-minded, or imbecile, rather than belonging to the lower grades of idiocy.

It may be noticed, that some writers have seriously advanced that a violent blow or concussion upon the head has occasionally effected a cure of idiocy or gross obtuseness of intellect. This is naturally a very rare event, even in upper-class schools, and the instances that are given of it do not appear to me to be always trustworthy. I have seen it, for example, asserted in several books[1] written by physicians of reputation, that Father Mabillon, a celebrated writer upon ecclesiastical antiquities, was an idiot till the age of twenty-six, when he had the good luck to fracture his skull, was trepanned, and changed into a learned writer upon ecclesiastical history. This is not the only one of these medical anecdotes copied from one book into another, which cannot stand inquiry.

The following are examples of traumatic idiocy after the dangers attending birth had been passed:—

N. X. was the son of healthy parents, in whose family no infirmity of mind had appeared. He began to walk and speak at the usual time, and was a thriving, intelligent child. When four years old he got a fall from a pony, lighting upon the head. There was a depression stretching from the nasal eminence to the middle of the forehead, one and a half inch long, marking the seat of injury. He grew up to be a strong young man, with a head of good size. He could read and write, and had some knowledge of arithmetic. He was fond of reading amusing books like the Arabian Nights. He was garrulous and frank in conversation, and not very easily managed. He was distinctly simple-minded, not having more intelligence than a child of eight years of age.

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[1] See, for example, Dr Forbes Winslow's Obscure Diseases of the Brain and Mind, London, 1863, p. 371.

B. N., though late in walking and speaking, was not observed to be idiotic till after a fall on the crown of the head. A cart, which she was in, was upset, and she was found under, stunned. There is no record of idiocy or insanity in the family. She is well-made, and comely in appearance. She is now sixteen years old. She can read little stories for her own amusement, and can do some household work. She may be expected to go on improving, having been only eighteen months under training.

(*To be continued.*)

ARTICLE VII.—*Death from Inhalation of the Products of Combustion, in an Open Fireplace with a Chimney.* By JOHN BALFOUR, Esq., I.G., Leven.

DEATH from inhaling the products of combustion in close rooms, cabins of small vessels, and similar places, is by no means unusual, but such an event taking place in a good-sized bedroom, with an open fireplace and chimney, is so uncommon that I think it worthy of being placed on record.

On the night of Sunday, the 23d of November, James Carstairs and his wife, both strong and healthy, partook of a slight supper with some friends, the meal consisting of fish, a little cheese, bread, etc., and about half a tumbler of porter. Mr C. retired to his room about midnight, and his wife shortly after. A fire had been lighted some time before in the room, and was then in a clear red state, without flame. Not having made their appearance, they were called a little before nine on Monday morning; at first without answer, then with an indistinct reply from Mrs C. that they would not rise yet. Something being supposed to be wrong, the door was opened, when both were found lying on the floor: Mr C. on the hearthrug, with his head near the fireplace, with a quantity of vomit on the floor near him; Mrs C. farther away from the fire, and with her head under the foot of the bed. Both were at once raised; Mrs C. was put on her own bed, and Mr C. removed to a bed in another room; and I was sent for. On my arrival, which was about ten minutes past nine, I first saw Mr C., and found life extinct. The heart had ceased to act, and the extremities were becoming cold. I then saw Mrs C. She was scarcely able to speak, pulse about 150 and very weak, much oppression in breathing, vertigo, extremities very cold, and much shivering.

Hot bottles were applied at once, a little spirits and water given, and, as soon as possible, a mixture containing a little sp. ammoniæ aromat., with a few drops of morphia and digitalis, was prepared for her. During the day she rallied much, and was then able to state that some time during the night (she was quite unable to guess what the hour was) she awoke feeling very unwell—great difficulty in breathing, heart beating rapidly and with difficulty. She awoke her husband, and asked him to call up some of the family. He told her to keep quiet; she would soon be better. Somehow or other (she did not know how) she got out of bed and staggered about, falling on the floor. Her husband rose to aid her; tried to light the gas, but failed, and fell on the floor, and she remembered nothing more. The bed was 2 feet from the floor, the bedroom measured 15½ feet by 12½, and 8 feet high. The gas bracket was all right when I examined it, the fire had gone out, the damper of the grate was up, and the grate contained cinders with little or no ash. Everything appeared to point to poisoning by carbonic acid; but the case being a very unusual one, it was reported to the Fiscal, and a warrant was issued for the examination of the body. A copy of the report made by Dr Fraser (of the Fife and Kinross Asylum) and myself is appended.

I have only to add that, the house being new, both door and window fitted very closely; and that the night being a very stormy one, and the fire small, it is probable that the wind, blowing sharp and steady over a low chimney, acted as a damper, and effectually prevented the gases from making their escape by the way provided for them.

"LEVEN, *November* 25, 1873.

"We hereby certify, on soul and conscience, that we this day, Tuesday, 25th November 1873, examined the body of James Carstairs in the house of his brother, Mr W. Carstairs, and that the following is our report of the case and opinion thereanent:—

"The body was wrapped in a sheet, inside which was found an ordinary white shirt.

"*External Appearances.*—The body was that of a full-grown young man, and was in good condition. The face was slightly swollen, and there were patches of lividity about the forehead, cheeks, nose, and mouth. The mucous membrane of the mouth was of a dark livid colour. Large patches of lividity existed on the upper and anterior parts of the chest. Hypostasis very well marked posteriorly. Cadaveric rigidity very great. Thorax.—Section of skin over anterior wall was attended with a flow of venous blood. Heart, right side, distended, with dark, semi-coagulated blood; left side empty. *Valves, cavities, and substances quite healthy.* Lungs.—Left, old adhesions posteriorly. The whole organ was of a dark livid colour, deeper posteriorly. On section, its substance was found hyperæmic, especially posteriorly. The mucous membrane was swollen, and of a dark-red colour; its surface was covered with a frothy, sanguineous secretion. Right lung.—The same in every particular.

"The tongue, larynx, trachea, and œsophagus were removed *en masse.* Mucous membrane of mouth, tongue, posterior part of pharynx, larynx, and trachea, was intensely congested, but that of the œsophagus was normal. A slight coagulated mass of dirty mucus was found at the superior laryngeal orifice, evidently the remains of a vomit. Fine frothy mucus projected from each nostril.

"Abdomen.—Nothing abnormal seen on opening the cavity. Stomach contained about half an ounce of thick fluid, of a dark-brown colour, with a greenish tinge. It smelt of, and contained, fragments of cheese. The mucous membrane was slightly hyperæmic along the rugæ. Alimentary tract normal.

"Liver somewhat full of venous blood, but healthy. Spleen and kidneys normal. Bladder very much distended with healthy urine.

"Head.—Hair was matted together with vomits.

"Brain.—Membranous veins of arachnoid distended anteriorly, and very much more so posteriorly. Brain normal in size, configuration, and consistence. On section, puncta cruenta were more marked and numerous than usual. *Substance healthy, and ventricles normal.*

"The following are our conclusions:—

"1. That the body was that of a healthy man.

"2. That the pathological appearances present were those always found in cases of poisoning by carbonic acid gas.

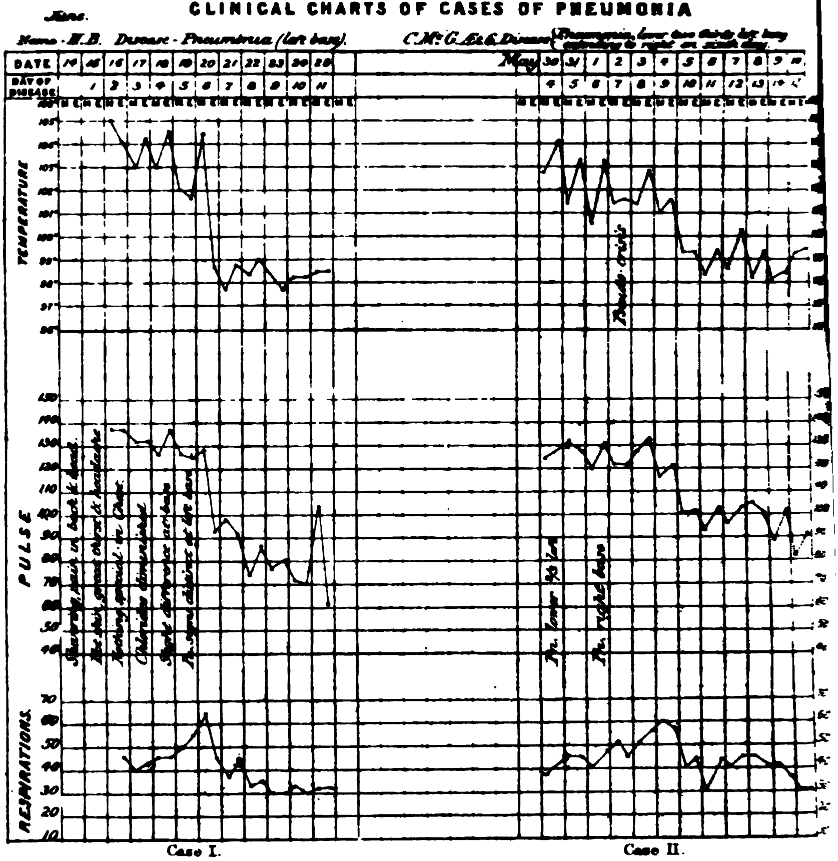
"3. That from these appearances, from the inspection of the room, and from the description of the symptoms given by the widow, who herself nearly lost her life, we are of opinion that death resulted from poisoning by the inhalation of carbonic acid gas, generated by a smouldering fire in a close room.

"JOHN BALFOUR, F.R.C.S.E., I.G.
"JOHN FRASER, M.B., C.M."

ARTICLE VIII.—CHILDREN'S HOSPITAL PAPERS. No. I.—*Clinical Observations on Pneumonia.* By WILLIAM STEPHENSON, M.D., F.R.C.S. Ed., Physician to the Royal Edinburgh Hospital for Sick Children.

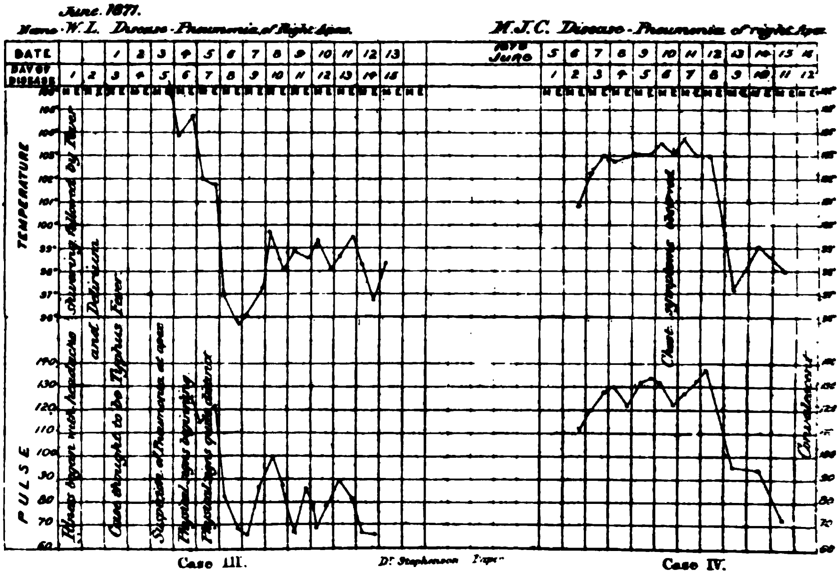
WHEN once a medical man becomes familiar with the thermometer in practice, he soon finds it an essential to his comfort, and feels as uneasy without it as did our fathers when their lancets were not in their pockets, or we ourselves are without our stethoscopes. Those,

CLINICAL CHARTS OF CASES OF PNEUMONIA



Case I.

Case II.

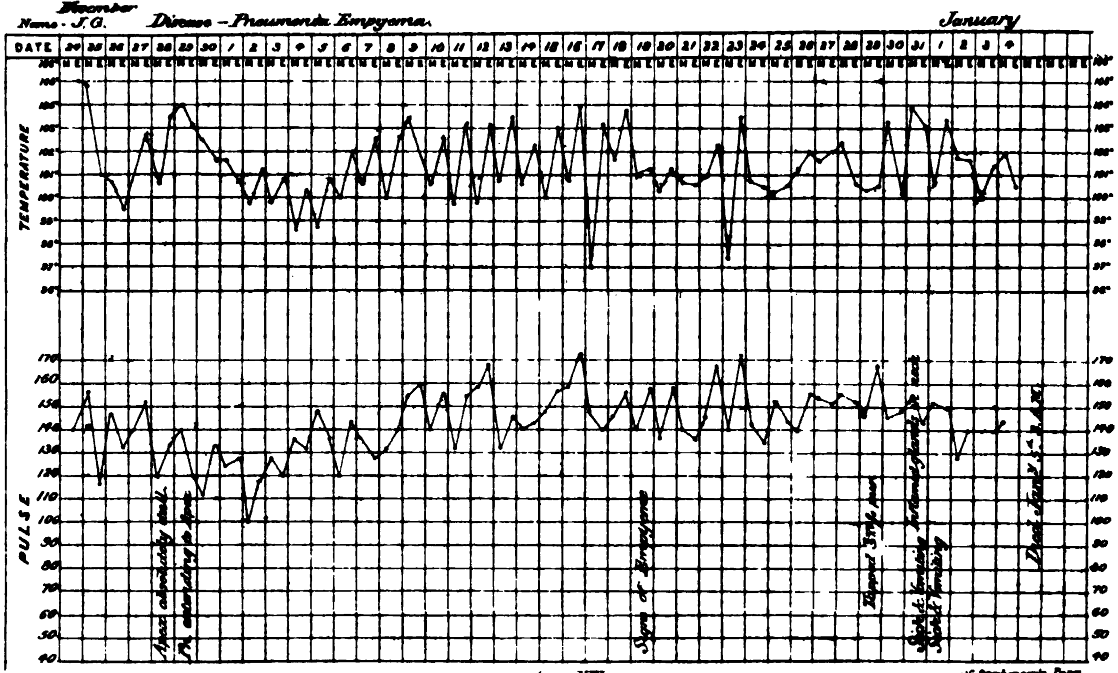


Case III.

Dr Stephenson Paper

Case IV.

CLINICAL CHARTS OF CASES OF PNEUMONIA.



Case VIII.

however, who have not had the benefit of an hospital appointment, and, still more, who are largely engaged in practice, find some difficulty in studying the subject with that amount of fulness and detail which is essential to its thorough appreciation. Our systematic works do not yet fully reflect the subject in all its bearings, and in books on medical thermometry it is too much generalized, whilst the physician has to particularize, and feels the want of the comparison of the pulse and physical signs with the thermometric details before him.

For these reasons the following cases, selected from among those which have occurred in my practice at the Royal Hospital for Sick Children, may prove of interest to many. Although possessing no single feature which has not been observed before, they will illustrate some points which have not received that attention which they merit.

In the study of pneumonia, as well as other acute diseases, considerable advantage is to be found in taking the clinical chart as the true representative of the disease, and of higher value, both practically and scientifically, than the auscultatory signs or pathological products. The two never run counter; but in the light of the scientific precision and accuracy of record obtained by the former, the disease stands out in bolder relief and clearer outline during the lifetime of the patient than was possible before the introduction of the thermometer. By its aid we have an invaluable guide to diagnosis and treatment, and a surer test of the results of that treatment, than can be obtained from the physical signs alone.

Regarded in this light the latter become, in a great measure, merely the confirmatory evidence for the interpretation of the general state—the signature whereby the record is to be attested. But in all the acute febrile diseases it is with the amount and character of the pyrexial state which we, as physicians, have most to deal. It has been found that many have a definite and constant character, and we are thereby placed all the more independent of auscultatory evidence—a matter of no little importance where that may be for a considerable time latent or equivocal.

The diagnostic evidence so gained has a very special value in the diseases of children, from the peculiarities which the physiological characters of that age impart to the various affections classed under the term pneumonia. Considerable difficulty at times is found in early determining the nature of the affection. This frequently arises from certain peculiarities which are more frequently met with in children than in adults. Those to which I would specially refer, as not having received a due amount of attention, are the following:—The greater frequency of cases where the physical signs remain latent for a longer period than is usual in adults; the occurrence of cases where the physical signs throughout are slight; and the greater frequency of the affection, attacking primarily and limited to the apex. These peculiarities are mentioned by writers, but only casually, whilst the frequency with which I have met them has given to them a greater importance to my mind, and has led me to suspect that many cases of pneumonia are overlooked or mistaken for simple febrile attacks.

The text-books, as yet, recognise only two forms of pneumonia as occurring in children—the primary, acute (croupous), or lobar, and the lobular (catarrhal) or broncho-pneumonia. The pathological differences between them are clearly stated, but they are still treated of too much together, whilst the principal clinical distinction lies in the secondary character of the lobular form, with a greater obscurity or uncertainty in the physical signs. The subject, however, has entered on a more advanced stage. Cases of acute primary pneumonia are resolving themselves into different types, when viewed in relation to temperature, pulse, and physical signs; catarrhal pneumonia has been recognised as a primary affection, unassociated with bronchitis; and cases complicating affections of the bronchi may be either broncho-pneumonia, or bronchitis with true lobar or croupous pneumonia.

Whilst so much foundation work remains to be laid in the differentiation, it is not to be wondered that the question of treatment should be in so unsettled a state as we find it. No true advance, moreover, is ever likely to be made so long as cases are aggregated together by the hundred, with only the one connecting link of the physical signs characteristic of the general term pneumonia.

At present I shall deal only with the class of the acute primary affection, leaving for subsequent papers the consideration of the other forms. The cases are selected to illustrate the points already referred to, while at the same time they show the relation of temperature and pulse as it bears upon prognosis and treatment.

The characters which mark the true acute pneumonia are—the immediate development of a high pyrexial state, which runs a pretty even course, showing but slight tendency to remission for five days, then declining slightly for one or more days, and terminating by a well-marked crisis.

When chest symptoms are present little difficulty of diagnosis is met with; but when unaccompanied by such, the diagnosis turns between it and fever of some kind, and mistakes in this direction I have found made from cases being sent to the hospital as fever. Scarlet fever is the only form in which the high pyrexial state is so immediately developed which is likely to raise a doubt, whilst the absence of sore throat and non-appearance of the rash should soon remove it. In such cases—that is, where there is absence of cough, pain in the chest, or marked alteration in the respiration—there is one symptom to which experience has led me to attach much importance, and that is the presence of delirium during sleep. In children above two years of age I have never found it totally absent, since my attention has been specially directed towards it as a symptom in the class of cases I am speaking of; and on several occasions its presence has enabled me, by careful watching, to discover evidences of pneumonia of very limited extent. Its presence, moreover, at the onset, tends to negative the supposition of typhus or typhoid, as in these fevers it is a later symptom. I am fully aware that indigestion, or other trifling cause, will often make a child feverish and talk in his sleep, but I here speak of the delirium only as a symptom to place us on our guard, and in the absence of special indications to direct attention to the chest.

Whilst there may be an absence of *marked* alteration of respiration, a careful examination, especially of the naked chest, seldom fails to detect some *minute* change. The rate is quickened, but increased rapidity is common in children to pneumonia and the purely febrile state, but in the former it is more shallow, and later in the affection becomes more and more abdominal. In young children the audible respiratory character is of great importance. In Case V. there was observed a slight indrawing, with respiration, at the root of the neck, before the costal movements were arrested.

While watching such a case, with a temperature from 103° to 104° from the first day, and on the fifth to the seventh day a marked crisis occurs, I would certainly believe it to have been an acute inflammatory chest affection, even although I failed in detecting any physical signs, blaming my auscultatory powers rather than distrusting the evidence before me. Case V. illustrates how slight the physical signs may be, and I believe it is by no means rare for children to have a very limited amount of lung lesion, while the physical state is exceedingly well pronounced. In a case which occurred in private practice, where, from the character of the pyrexia and the presence of delirium, but without the slightest chest symptoms, I kept a strict watch over the whole chest, I found on the fifth day, limited to a spot in the upper right lung, in front, covered by the end of the stethoscope, distinct crepitation and bronchial breathing, without any change in percussion. Next day not a trace of lung lesion could be detected, whilst a marked crisis had occurred during the night. I need not remark how anonymous this febrile attack would have been had the signature not been thus appended.

Pneumonia, attacking primarily and limited to the apex, is much more frequent in children than in the adult. It is necessary, therefore, to auscultate this part as carefully as any other. My experience confirms that of others, that in this class the immediate high temperature and delirium is very pronounced. In Case III., which had been stated by a medical man to be fever, I expressed my opinion, from the pungent heat of skin and marked delirium, that it would prove to be pneumonia of the apex, and admitted him accordingly to the general ward. In this class of cases the importance of the general clinical characters is enhanced by the fact that in them the physical signs are, as a rule, long latent (fifth day), and the affection often very limited. To this there is an exception, as I shall afterwards show, of an exceedingly fatal type, where, from the first, the consolidation of the lung is very great in degree. As a rule, well-marked delirium and high temperature, with absence of other indications, should always direct attention to the apex of the lungs, and a daily examination be made, otherwise the confirmatory evidence may escape notice. It should also be remembered that in young children convulsions may take the part of delirium.

CASE I.—*Pneumonia, left base—Physical signs on fifth day—Crisis at the end of sixth day, with exacerbation during seventh day.* (*See Chart.*)

Henry B., æt. 8, a well-nourished boy. Well till evening of 14th June, when he had shivering, pain in back and head. 15th, hot skin, great thirst and headache. 16th, admitted to hospital.

	Day of Illness.	T.	P.	R.	
June 16	II.—E.	104	136	48	Skin hot and dry; occasional short cough; rapid respirations; no pain anywhere; tongue coated, papillæ prominent; heart sounds, normal; nothing special in chest
„ 17	III.—M. E.	103 104·4	132 134	40 44	Restless night, no delirium; slight headache; bowels moved; at right apex, respiratory murmur deficient, expiration prolonged, almost bronchial; no dulness.
„ 18	IV.—M. E.	103 104·6	128 138	44 44	Talked during sleep; face flushed; apex as yesterday; slight difference in percussion and respiration at the base, posteriorly, but not very definite; chlorides in urine diminished.
„ 19	V.—M E.	102 101 8	128 124	50 55	Dulness, tubular breathing, and increased vocal resonance at left base, more marked in the evening; anteriorly, no dulness; harsh breathing.
„ 20	VI.—M. E.	104·6 98·7	128 92	64 45	No marked change in physical signs; no moist sounds; slight frontal headache.
„ 21	VII.—M. 2 P.M. E.	97·7 103 98·8	98 ... 92	38 ... 44	Tubularity not quite so great; exacerbation of fever in the middle of day; evening better, a few moist râles below angle of scapula.
„ 22	VIII.—M. E.	98·5 99	75 88	32 35	Slept well; tongue cleaner; less dulness at base; very slight crepitation on deep inspiration; above spine of scapula, breathing slightly bronchial; expiration distinctly marked; no dulness; anteriorly, breathing harsh.
„ 23	IX.—M. E.	98·5 97·8	78 80	30 30	Râles at base more distinct; no other change.

24th.—Dulness now quite gone; moist râles, and slight increase of vocal resonance from spine of scapula downwards; at the extreme base there is a marked decrease of vocal resonance.

29th.—The lung has gradually cleared up, and this morning there was only slight harshness of breath sounds and increase of vocal resonance. Dismissed quite well.

CASE II.—*Pneumonia of left base, extending to right base on fifth day—Crisis prolonged from seventh to the tenth day.* (*See Chart.*)

Catherine M'Garry, æt. 6, has been a delicate child, liable to coughs and bronchitis. Was quite well on 26th May till the evening, when she had rigors, having overheated herself during the day; 27th, hot and feverish, with short cough, pain in the side, and vomiting.

	Day of Illness.	T.	P.	R.	
May 30	IV.—E.	104°	130	38	Dulness, with tubular breathing over lower two-thirds of *left* lung; no râles; milk and beef-tea; poultices to left side.
„ 31	V.—M. E.	101·5 103·4	130 128	44 44	Left side as yesterday; slight dulness and harsh breathing at right base; vespere, occasional moist râles on left.
June 1	VI.—M. E.	100·5 103·2	120 130	40 48	Tubular breathing at *right* base, posteriorly; râles coarser on left; pain in right side.
„ 2	VII.—M. E.	101·4 101·5	124 124	50 45	Subcrepitant râles on both sides; breathing still tubular.
„ 3	VIII.—M. E.	101·4 102·7	128 132	50 55	No marked change.
„ 4	IX.—M. E.	101 101·4	115 120	60 55	Physical signs the same; to have sherry 4 oz., quinine ij. gr., c. tr. scillæ, ter in die.
„ 5	X.—M. E.	99·7 99·3	100 100	40 44	Dulness not so great on left side.
„ 6	XI.—M. E.	98·5 99·5	95 102	30 42	Râles very coarse and abundant.
„ 7	XII.—M. E.	98·7 100·1	98 102	40 44	Left side now as clear as right, though both slightly dull.
„ 8	XIII.—M. E.	98·2 99·3	104 100	44 40	*Diarrhœa* slight this morning (bowels previously moved regularly and naturally).
„ 9	XIV.—M. E.	98 98·2	88 100	40 38	
„ 10	XV.—M. E	99·4 99·5	82 90	30 30	Râles continue; no dulness; cough not so troublesome.

Allowed to get up on the 16th; by the 25th all trace of chest affection had disappeared. The chlorides in urine were never markedly diminished during the time she was under observation.

CASE III.—*Pneumonia, right apex—No definite physical sign till sixth day—Marked crisis on seventh day.* (*See Chart.*)

William L., æt. 7½, admitted to Children's Hospital 3d June 1871. Previous to present attack was in good health; illness began on Tuesday, 30th May, with shivering and headache, and acute delirium, followed by high fever, which has continued ever since. The case, when seen out of doors, was supposed to be typhus fever.

	Day of Illness.	T.	P.	
June 3	V.—E.	106°	124	On admission, the skin is felt to be hot and dry, the tongue moist and thinly furred, the face dark and typhus-like in expression; examination of the chest showed nothing abnormal, except just a suspicion of pneumonia at the apex, but no distinct physical signs can be made out.
„ 4	VI.—M. E.	103·8 104·8	124 124	During last night child has been very delirious; he is very thirsty, and drinks freely of milk and beef-tea. *Chest.*—Respiration rapid and shallow, almost entirely abdominal; tremulous pulsation visible over the large vessels in the neck; anteriorly, a shade of dulness on right side in the acromial half of the subclavicular region; occasional fine crepitation, with sibilant râles over the whole right front; over the upper lobe the respiratory murmur is deficient, with conveyed bronchial sound; in the lower lobe, breathing is exaggerated; posteriorly, percussion natural; exaggerated vesicular breathing; no râles.
„ 5	VII.—M. E.	102 101·8	112 120	Still delirious; lips and teeth coated with sordes; respiration less abdominal, still shallow, quick and uneasy; marked dulness in right subclavicular region; respiratory murmur faint; subcrepitant râles; towards base, exaggerated breathing; scanty râles.
„ 6	VIII.—M. E.	97 95·8	85 68	Physical signs much the same; râles less abundant, heard occasionally at back.
„ 7	IX.—M. E.	96 97·5	65 88	Still improving; appetite good; chest the same.
„ 9	X.—M. E.	98·7 98	65 88	Dulness diminished; no râles. From this date chest symptoms steadily cleared off. Patient dismissed 19th June.

The next case was sent to the hospital as a case of fever; an opinion which the apparent gradual rising of temperature during the second and third day of illness, and the absence of all chest symptoms, confirmed until the sixth day, when the pneumonic expression, quickened breathing, and cough, drew attention to the chest. On entering the ward the house-surgeon remarked—"Does not that child look as if she had pneumonia?" He had examined the chest, but having only directed his attention to the lower lobes, had failed to detect the physical signs.

CASE IV.—*Pneumonia of right apex—Mistaken for fever—Crisis at the end of the eighth day.* (*See Chart.*)

Mary Jane Cochrane, æt. 10, admitted 6th June; taken ill the day before.

DAYS,	II.	III.		IV.		V.		VI.		VII.		VIII.	IX.	X.	XI.
	E.	M.	E.	M.	E.	M.	E.	M.	E.	M.	E.				
T......	100·8	102·4	103	102·9	103	103·1	103·1	103·5	103·1	103·6	103	103	97·2	99	98
P......	110	120	128	130	124	132	136	138	124	128	132	138	98	95	72

Sixth day, 10th June.—To-day, patient presents a well-marked pneumonic flush of face, the respiration has quickened, and she has, for the first time, a short troublesome cough. Examination of chest shows dulness over upper lobe of right lung, front and back; anteriorly great deficiency of respiratory sound, but copious râles are heard with each cough; posteriorly, tubular breathing and subcrepitant râles on deep inspiration.

Seventh day.—Tubular breathing, but no râles.

Ninth day.—Distinct crisis; T. 97·2, P. 98; at apex respiratory murmur much softer; expiration prolonged and tubular; under second rib respiration very faint and tubular.

The lung gradually and rapidly cleared up; she made a good recovery, but while in the ward she had absorbed the typhus poison, which manifested itself on 30th June. It was mild in form, and she was dismissed well, 30th August.

CASE V.—*Pneumonia—Right apex—Very slight physical signs—Protracted convalescence—Bi-hourly observations of temperature.*

John Gill, æt. 6 years, admitted 8th May 1872, suffering from want of power in the left leg; a healthy-looking boy, but was delicate until 3 years old. In January had what mother thinks was a rheumatic attack, after which the left leg was noticed to be weak. Thursday, 16th May, patient was allowed out of bed. At night he became out of sorts, and on Friday morning he said he preferred to keep his bed. His skin, however, was cool. At night he became flushed and skin hot. Saturday, 18th May, the skin was again cool, but he became heavy, and the feverishness again returned in the course of the day. Was prescribed by the house-surgeon tincture of aconite, one minim every hour.

19th May.—Drowsy and languid; talks in his sleep; cheeks flushed; skin dusky. Respiration is quickened, and each inspiration is accompanied by slight indrawing at the root of the neck. Costal and abdominal movements natural. Some slight cough. A careful examination of the chest failed to detect anything decided. It was recorded, but as an impression only, that the vesicular murmur was somewhat increased at the right apex, posteriorly and anteriorly a slight degree of increased resistance. At the same part, on auscultation, the respiratory sound was of the same character as on the opposite side, yet it appeared to be more distant from the ear.

20th.—Cheeks still flushed at times, but the colour often passes off, and the face assumes a degree of pallor. He lies upon his back, and for the most part seems to be sleeping, but is readily roused. Respirations 40 per minute, and almost entirely abdominal. The indrawing at the neck is somewhat increased. He complains of no pain upon inspiration. Auscultation still reveals nothing distinct. The increased puerile respiration posteriorly certainly does not exist to-day, but in front the impression of yesterday is confirmed, though not increased. Urine, sp. gr. 1025; colour orange; considerable deposits of urates; chlorides diminished; no albumen.

21st.—Condition much as yesterday, except not so drowsy. Still talks in his sleep. Respiration 44, still abdominal. Bowels moved once. Tongue clean and moist.

At one spot under left clavicle there is heard occasional, distant, subcrepitant râles, and some sibilus; nothing else.

22d.—Continues much the same as yesterday.

23d.—Decidedly better. Respirations quiet and easy. He looks about him and amuses himself, but at times gets heavy and drowsy. The temperature has fallen, but shows a tendency to rise. *Auscultation.*—At right apex no râles are heard, but there is a less volume of respiratory sound as compared with the other side, with slightly increased resistance. Stop the aconite.

24th.—Continues easier, but has regular accessions of fever. A point of dulness at sternal end of first intercostal space. To have sherry iv. oz. per diem.

25th.—In the same condition. To have 4 grains quinine every six hours.

27th.—Febrile condition diminishing. Plays a little, but often relapses into sleep. Has a moist cough. Moist sibilus, and has occasional râles over the whole chest.

From this date the temperature kept normal, and the child was removed on 1st June from the hospital against my wishes. The cough, with slight sibilus, continued, but the child's general state was improving. He was beginning to take some food.

Bi-hourly Observations of Temperature in the Case of John Gill.

	A.M.						P.M.						Days of Illness.
	12	2	4	6	8	10	12	2	4	6	8	10	
May 19	...	...	...	...	103·4	...	...	...	...	...	104	105·5	III.
„ 20	105·2	104	104·2	103·8	...	...	104·9	105	...	104·4	104	104·4	IV.
„ 21	105·2	103·6	104·2	102·4	102	101	102·5	104	103·8	103·8	103·4	103	V.
„ 22	103·4	104·2	104·2	101·8	100·6	102·2	101·8	103·8	104·2	102·6	104·8	103·8	VI.
„ 23	103·4	104·2	105	102·3	102	102·4	99·6	100·8	101·8	101·6	101·4	102	VII.
„ 24	102·8	102·4	103·2	101·3	101·6	101·4	103·8	104·2	101·2	97·8	100·8	103·7	VIII.
„ 25	102·5	102·4	100·8	100·8	99	101	103·2	104·8	102·8	104	104	...	IX.
„ 26	102	100·3	100·3	100·3	100·4	94	100·2	100·6	104	101·2	100·6	99·4	X.
„ 27	99·3	99·2	100	100·4	99	99	100·8	103·2	101·9	98·6	97·6	98·2	XI.
„ 28	99	100·3	99·6	98·8	98	99·6	102·2	98·2	99·2	99	97·6	99·2	XII.

CASE VI.—*Pneumonia, right apex, fatal in 24 hours—Physical signs of complete consolidation from the first.*

Philip Ross, æt. 2½, admitted 21st Nov. 1873.

Patient was running about the house, and seemed quite well yesterday till night, when he was suddenly seized with severe shivering and vomiting, quick breathing, and high fever, with a good deal of wandering. When seen at home (by Dr Stephenson) at 11 A.M. the skin was pungently hot, his breathing rapid, with audible expiration. The child was exceedingly restless. A hurried examination of the chest revealed perfect dulness at right apex anteriorly. Posteriorly there did not seem to be any dulness, but further examination was prevented by the restlessness of the child.

After admission—(4.30 P.M.).—Pulse 144, small and feeble; respirations 70 per minute, audible and expiratory in character; skin feels cool, face pale, no flush.

Chest, front, right side.—Dulness absolute at apex, less so below nipple, clear in the axillary region; bronchial breathing throughout. *Left.*—Percussion natural, harsh vesicular breathing, no râles. *Back, right side above spine of scapula.*—Dull, but not nearly so decided as in front; respiratory murmur over the same region also bronchial, but not so harsh as in front; the rest of chest is resonant, breathing harsh, vesicular; some coarse moist râles at left base. Patient inclines to doze off asleep, but starts up every few minutes in great alarm. Ordered mixture containing citrate of ammonia and sp. chloroformi. Hot fomentations to chest. Four ounces sherry to be made into wine-whey.

Temperature in axilla, half an hour after being put to bed, was 99·4°; at 5 P.M. 104·3°.

During the afternoon the breathing continued much the same, until a few minutes past 8 o'clock, when the nurse, while applying fresh fomentations, observed that the breathing was very calm, and getting slower. The house-surgeon having been summoned, he found the respirations were feeble, the eyes squinting, pupils dilated, hands clenched, and all the muscles in a state of fixed contraction. In this state the child died at 8.40.

Post-mortem Examination (*by Prof. Sanders*).—Heart—pericardium healthy, right auricle and ventricle full of blood, left auricle and ventricle empty. Left pleura healthy, left lung congested, otherwise healthy. Right pleura contained about 10 oz. clear serum. Pleura pulmonalis and costalis covered with white exudation bands, which unite the surface of the lungs to the chest-walls superiorly and posteriorly. Upper lobe swollen and hard (pneumonic); lower compressed, but soft. On section of lung there are found in upper lobe, towards the anterior margin, circumscribed patches from the size of a pea to an almond, consisting of soft white exudation; the larger, of red hæmorrhagic exudation. Liver and spleen healthy. Kidneys congested; otherwise healthy.

This case belongs to a type of the disease which is not uncommon in children, and which, I think, ought to be clinically disassociated from the others. The example given is exceptional in the extreme rapidity of the fatal termination; but I have met with several cases possessing the same characters, all of which proved fatal within three, or at most four, days. They are marked by the intensity of the general symptoms, dyspnœa and nervous derangements, but not necessarily a higher range of temperature than is met with in the favourable type; at the same time, however, the pulse is unduly higher in relation to the temperature. But the most important difference lies in the extremely early consolidation of the lung; excessive in degree, though often very limited in extent. At the first onset, as in the above case, the dulness may be absolute, with clear, dry, tubular breathing, or a complete absence of breath-sounds. This marked difference, together with the fatal character, clearly point to a difference in the essential nature of the affection, although we are unable to say in what it consists; and in all deductions, as to treatment or otherwise, these cases must be regarded as belonging to a distinct type, and ought not to be combined with the others. Did they differ only in their early fatal termination, we would be justified in concluding that the cause lay in the too great dynamic blow received by the nervous system; and this may indeed be true, but we have likewise to explain the marked difference in the lung lesion. The next case also belongs to this type. It was associated with an extreme degree of blood-changes, which would sufficiently account for the difference; but such is not always the case. Philip Ross appeared in good health up to the attack, and there was no perceptable change in his blood. I have met with the affection in a fine healthy child, nine months old, where it was apparently caused by his having been kept out late the previous evening. In the morning, after a restless night, he was seized with violent convulsions, and in the afternoon I found complete consolidation over the whole of one lung. He died the following day, the second of the illness. I can recall to memory other cases in healthy children, so that neither a cachectic state, nor any apparent blood-poisoning, can account for the special features of this type. Whether pathological histology will ever throw any light on the subject, I cannot say, as it is only of late I have been led to disassociate these cases from the other kinds. But to those who would object to the grounds I have given, as insufficient to found any difference upon, I would remark that all the varieties of pneumonia have special clinical features by which they can be recognised, and their clinical study and pathological research have gone hand-and-hand together, now one and now the other leading the way to their differentiation.

CASE VII.—*Pneumonia of right apex, with marked blood-disorganization, fatal in three days.*

William Douglas, æt. 13, an inmate of the United Industrial School; a healthy boy; body well nourished. He went about the school as usual, and made no complaint on Thursday, 4th April 1873; but now says he felt "sick and unhappy." On Friday morning he vomited, and did not rise with the other children. He continued to vomit occasionally during the day, but there were no symptoms other than an ordinary attack of deranged stomach, and a powder of compound rhubarb was administered. On Saturday morning, when the sergeant visited him, he was struck by his prostrate condition, and dark colour of the point of his nose. The vomiting had continued at short intervals. When seen by Dr Stephenson, he was at once removed to the Children's Hospital.

State on admission.—Patient is in a state of great prostration; features are pinched; face sallow, with somewhat icteric colour of the skin and conjunctiva; a dark ring under both eyes; point of the nose bluish-black; the feet are also livid and ecchymosed in patches; nails of fingers dark, but not discoloured.

Eyes.—Right pupil, contracted; left, largely dilated; both, almost quite insensible to light; patient can tell the number of fingers held up equally with either eye.

Pulse.—Extremely weak and rapid, somewhat near 200.

Respirations.—Very irregular in frequency; sometimes deep, at other times shallow.

Chest.—At right side posteriorly there is complete dulness from the apex to a finger's breadth below spine of scapula, and over that region only a feeble respiratory sound is heard; below spine, breathing is bronchial in character; rest of chest, in a hurried examination, seemed normal.

Liver.—Dulness, normal; some tenderness on pressure over that region.

The mind is quite clear; no stupor. Temp., 103·8°; pulse, 180.

Ordered.—℞ Tr. ferri mur., ʒiv.; potass. chlorat., ʒiij.; aq. ad ℥vj. Half an ounce every quarter of an hour, for one hour; afterwards, every hour. Brandy and white of egg with phosphate of soda and chloride of sodium, well mixed, in half-ounce doses, frequently. An enema to be administered.

6th April.—Has been restless and delirious all night; bowels moved with enema; everything taken has come up; condition in every way the same; enemas of beef-tea and brandy have been given, and only brandy and soda-water by the mouth. In the
T. M. 99·6 afternoon the vomiting was not quite so frequent, but
E. 100·5 prostration more marked; extremities, cold; fingers,
P. — + 180 — livid; urine, albuminous.

7th.—Passed an extremely restless night; delirium continues, but the boy answers questions readily; vomiting stopped at 1 A.M.; enemas have since been discontinued, beef-tea and brandy being retained by the stomach; prostration increased, and patient sank at noon. The blood was examined microscopically during life; the red corpuscles were pale in colour, swollen and sticky, not forming into rouleaux.

Autopsy.—Old fibrous adhesions of both pleuræ; left lung congested in inferior lobe; upper lobe, natural.

Right Lung.—Upper lobe in a state of gray purulent hepatization; lower lobe, congested; other organs, healthy.

CASE VIII.—*Pneumonia of left base, extending upwards, and finally involving the whole lung—Empyema—Paracentesis—Death.* (*See Chart.*)

John Gifford, æt. 4, admitted 24th November 1870. Patient left the hospital on 14th November, being then convalescent from a mild febrile attack; has been feverish, with cough and difficult breathing, for about four or five days.

	T.	P.	
Nov. 24—E.	105	140	Considerable cough and dyspnœa; audible expiratory respiration; marked dulness; bronchial breathing at left base; slight dulness at right base.
„ 25—M.	104·8	155	
E.	101	115	
„ 26—M.	100·8	148	* *The distinct fall of temperature shows an effort towards crisis, probably the seventh day; but a rise again takes place, with the extension of the inflammation to the upper lobe.*
E.	99·4	132	
„ 27—M.		140	
E.	102·8	150	
„ 28—M.	100·5	120	Dulness has extended over whole left side, but slighter at the apex than base; breathing bronchial throughout; right—harsh, vesicular, with coarse rhonchus; urine very scanty, contains a slight amount of albumen.
E.	103·5	135	
„ 29—M.	104	140	Absolute dulness at apex; intense bronchial breathing; respiration still expiratory; urine still scanty—albumen, a trace.
E.	103·8	120	
„ 30—M.	102·6	112	Patient seems worse; breathing very rapid, with increased expiratory character; physical signs remain the same.
E.	101·6	132	
Dec. 1—M.	101·6	124	Breathing more easily; seems better, still restless and uneasy.
E.	100·8	128	
„ 2—M.	99·8	100	* *Again an effort at crisis, with improvement in general condition; the evening temperature and high pulse show that general appearances are deceptive.*
E.	101·3	118	
„ 3—M.	99·9	128	
E.	100·8	120	
„ 4—M.	98·4	136	Since last date he has steadily improved in his general condition, sits up in bed, and takes his meals heartily.
E.	100·5	132	
„ 5—M.	98·8	148	Dulness still absolute at apex, but clearing slightly at base; respiratory murmur distinctly tubular, at apex seems distant.
E.	100·8	136	
„ 6—M.	100	120	
E.	102	144	
„ 7—M.	100·7	136	
E.	102·7	128	
„ 8—M.	100	132	* *A steady rise in both evening and morning temperatures has been taking place; and the pulse, always relatively high, is becoming to be out of all relation to the temperature.*
E.	102·7	140	
„ 9—M.	103·5	155	
E.		160	
„ 10—M.	100·7	140	
E.	102·7	155	

	T.	P.	
Dec. 11—M.	99·8°	132	Patient continues sometimes better, sometimes worse, still taking his food and brandy; the urine is greater in quantity, still slightly albuminous; respirations are still slightly expiratory; physical signs remain much the same.
E.	103·4	155	
„ 12—M.	99·8	158	
E.	103·2	168	
„ 13—M.	100·7	132	
E.	103·7	144	
„ 14—M.	100·5	140	
E.	102·3	144	
„ 15—M.	100	148	
E.	103	156	
„ 16—M.	100·8	150	
E.	104	172	
„ 17—M.	97	148	
E.	103·2	140	
„ 18—M.	101·5	144	Dulness still absolute in front, but only comparative behind; respiratory murmur still tubular in front, but at base posterior somewhat more healthy in character.
E.	103·8	155	
„ 19—M.	101	140	
E.	101·3	158	
„ 20—M.	100·3	136	
E.	101·4	160	
„ 21—M.	100·8	140	
E.	100·6	136	
„ 22—M.	101	142	
E.	102·3	168	
„ 23—M.	97·3	140	Upon examination to-day, there is discovered to be great bulging of left side, even up to the subclavicular region, with want of costal movement; the skin is slightly œdematous, and the intercostal spaces are not visible; cardiac pulsation is felt to the right of the sternum; on percussion there is absolute dulness.
E.	103·6	170	
„ 24—M.	100·8	142	
E.	100·6	136	
„ 25—M.	100·2	150	
E.	100·5	144	
„ 26—M.	101·2	140	
E.	102	156	
„ 27—M.	101·8	154	
E.	102·1	150	
„ 28—M.	102·4	154	
E.	100·5	152	
„ 29—M.	100·4	148	Seen in consultation by Drs Linton, Gamgee, and Stephenson; though deemed almost hopeless, it was thought proper that paracentesis should be performed, as the only prospect of affording any benefit; fifteen and a half ounces of healthy pus were withdrawn through a Bowditch syringe.
E.	100·5	168	
„ 30—M.	103·2	144	
E.	100·2	148	

	T.	P.	
Dec. 31—M. E.	104° 103·2	154 144	
Jan. 1—M. E.	100·7 103·3	150 150	Patient seems to have gained slightly in strength since the operation; his breathing is easier, and he is able to sit up in bed to take his meals; bowels natural; appetite improved; heart now beats under xiphoid; dulness continues over whole left side, but not so absolute in degree; child restless, and cries when auscultation is attempted.
,, 2—M. E.	101·9 101·8	128 140	During last 24 hours has only passed one and a half ounces of urine, highly albuminous; vomits his food, but retains egg-flip and brandy; there is a considerable fœtid discharge from his nostrils.
,, 3—M. E.	100 101·6	140 140	Is looking very feeble, but will still sit up in bed for a few minutes at a time; vomiting increased; nothing retained; urine, five ounces.
,, 4—M. E.	102 108	144 144	No urine to-day; beef-tea and brandy injections have been regularly administered, but he is fast sinking.
,, 5.			Died morning of 5th.

In this case we have an example of acute pneumonia running an unfavourable course. The child had been poorly nourished, and his health was further diminished by a previous peculiar and anomalous attack, with a high pulse, but a range of temperature at first only slightly increased, but afterwards below normal. He was evidently in a cachectic state before the attack. The exact date of the beginning of the pneumonia could not be ascertained. Upon examining his chart, we find a pseudo-crisis taking place, much more marked by the temperature than the pulse. An increase of fever occurred coincident with the extension of the consolidation to the upper lobe, and was again followed by an imperfect crisis. The temperature reached the normal only in the mornings of the 4th and 5th December; and though the general condition had improved, and he sat up in bed and looked about him, yet the steady rising of the pulse during this time showed the deceptiveness of appearances.

From the first, the range of pulse was out of proportion to the temperature, and kept this character throughout; thus indicating, far more than the temperature, the unfavourable type of the disease. From the 7th to the 18th December, the temperature chart presents the characters usually met with where morbid products are undergoing change. The empyema began after the 18th, as is evident by the examination of the chest on that day; and a distinct change in the character of the temperature is observed; the range keeping much more steady, indicative of the occurrence of a complication; the kidney affection was the immediate cause of death.

Since I began the practice of recording cases in the form of a clinical chart, the number of cases of acute pneumonia are too few from which to draw general conclusions, such as the proportion in which the apex is affected, the period of the manifestation of physical signs, and such like. Still, even the selection from them I have above made, are sufficient to illustrate some general observations. Much of the debatable ground with reference to pneumonia has been created by the want of the due recognition of the varieties included under that term; and much diversity of opinion, especially regarding treatment, has arisen from lumping together all the kinds, and the want of attention to the natural history of the affection.

It is clear that, taking the clinical chart as the proper representation of the disease, and not the mere auscultatory signs, that the true acute pneumonia is an affection which presents a well-marked typical chart, in which the temperature attains an immediate high range, and after running a pretty steady course, manifests, from the fifth to the seventh day, a distinct tendency to a crisis. From disturbing influences, however, the fever may be prolonged to considerably beyond that period; but when such is the case, the typical character is still manifested by a pseudo-crisis. This may be seen in the long-protracted and fatal case (No. VIII.), and in Case II., where, from extension of the inflammatory action, it terminated by a lysis, beginning on the fifth day, and extending to the tenth.

When due allowance, then, is made for disturbing influences, the affection must be regarded as possessing a strictly typical character, which differs in children in no way from the same affection in the adult.

Comparing the temperature and pulse range with the physical signs, no constant relation is perceptible, neither in time nor degree. An early consolidation does not hasten the crisis, and a very slight amount of lung-lesion is accompanied by the same typical character of fever as where a large portion of the lung-tissue is involved. The normal period for the fastigium appears to be five days, and during this time the febrile character is constant. It is only during the latter stage that some variation is met with, where disturbing influences have most power, and where the influence of treatment, it is probable, will become most apparent.

Since the introduction of the thermometer into clinical use, there has been a tendency to rely too much upon its evidence alone, as the test of the severity of the disease; and it is generally believed that the temperature in severe cases is always proportionately higher than in the milder forms. Such, however, is not an accurate expression of the case, if by severe we mean unfavourable. In Case III. (Lonie), the fever was sharp, and the highest temperature recorded above (106°) occurred in it; yet it was one of the most typical and favourable cases. Among the fatal cases, 105° is the highest temperature (Gifford). In Philip Ross it was 104·3°. When, however, we compare the relative range of temperature and pulse, a marked difference is found: 105° with a pulse of 140; 106°, pulse 124; 104° in the fatal cases corresponding to a pulse of 144 to 150; while in the favourable cases with only 130 to 136. It is therefore less by the height of the temperature, than by the relative range of temperature and pulse, that we are to form an opinion as to prognosis,—a point which has an important bearing on treatment.

Part Second.

REVIEWS.

Mind and Body: The Theories of their Relation. By ALEX. BAIN, LL.D., Professor of Logic in the University of Aberdeen. London: H. S. King and Co.: 1872. Pp. 196.

PROFESSOR BAIN is well known as a leading thinker and inquirer in what is held by some to be a heterodox school of philosophy. In adopting as the fundamental principle of his philosophical method and system the fact that the connexion of Mind and Body is not occasional or partial, as the orthodox school affirms, "but thorough-going and complete," he certainly adopts the best, if not the only fit, basis of an applied mental science.

After stating "the question" clearly in his first chapter, Professor Bain adduces, in his second, the usual facts in proof of his fundamental principles, and in his third chapter views the connexion of Mind and Body as "correspondence or concomitant variation." By this method we compare the gradations of the brain and the nervous system through the animal series, and observe whether there are like gradations in the powers of the mind. This is in substance the method of physiology in general and of phrenology; with such limitations, therefore, as the facts demand, Professor Bain endorses the conclusions of that system of psychology. This chapter applies facts, in elucidation of his method, drawn from recent researches into the histological structure and neurology of the brain.

In his fourth chapter, on the "General Laws of Alliance of Mind and Body," Professor Bain starts from the general fact already established, "that for every mental shock, every awakening of consciousness, every mental transition, there must be a concomitant nervous shock; and as the one is more or less intense, so must be the other." In developing this principle, however, he leaves the safe ground of correlative facts for the unstable domain of thought, and becomes at once confusing and obscure. "Mind," he says, "is now generally admitted to have a threefold aspect—three different functions—expressed by FEELING (including emotion), WILL or volition, and THOUGHT or intellect. These are a trinity in unity." The brain, it may be admitted, has its threefold functions, but mind is only an abstract term, expressive of certain cerebral functions in general, and cannot have a function at all in any scientific sense of the word. Again: "It is by combining the two general laws of mental and nervous action—relativity and diffusion," Professor Bain maintains, "that we obtain the comprehensive statement of its physical conditions of all consciousness—*an increase or variation of the nerve canals of the brain sufficiently energetic and diffused to affect the combined system of the out-carrying nerves, both motor nerves and nerves of the viscera*" (p. 57). So soon as we attempt to apply a general proposition of this kind to even so common a condition of consciousness as dreaming, we learn how futile it is. The successions of cerebral and mental states, as associations of ideas, are therefore declared to be "mysterious" in dreaming by comparison with those of the waking state. So, also, the inability to move experienced in dreaming terror finds no explanation in the combination of the two general laws which easily fits to the terror of the waking state, that "adds wings" to the flight of affright.

The laws of Pleasure and Pain, Self-conservation, Will, and Spontaneous energy, are considered in like manner with much ingenuity after a like method, not differing indeed from that of physiology in general, and of phrenology in particular. But the outline is more than usually vague, chiefly because Professor Bain has to take many of his physical facts at second-hand, encumbered too often with untenable hypotheses. Thus, he remarks, "We often see patients in the last stage of consumption still entertaining the most sanguine prospects of recovery; a proof that, instead of being mentally depressed, they are in the opposite, a joyous condition. On this it is remarked by Dr Patrick Nicol (Medical Reports of West Riding Asylum for 1872, p. 199), that blood from which tubercle is deposited, appears to have that peculiar injurious property for the brain *which excites delirium*,—in extreme cases it is productive of raving madness." Every statement in these sentences is either erroneous or hypothetical. Phthisical patients are not "often" possessed with the delusive feeling that they are stronger than they are in fact, and when so possessed are not necessarily—indeed rarely—in a "joyous condition." On the contrary, even with their delusive hopes, they are too often depressed and anxious; nor is there the slightest proof that tubercle as such is going the round of the circulation, or that the stuff found in the lungs in these cases is "deposited" from the blood. In truth, in certain delusively hopeful cases of this class the disease is not tubercular but pneumonic, although the general name given to it is phthisis; nor, lastly, is the delusive hope peculiar to this particular kind of pulmonary cases, or to pulmonary cases in general.

Chapter sixth deals with the knotty question, "How are mind and body united?" and Professor Bain argues well with those who object to any inquiries whatever, in answer to the question, on the ground that it is unsuited to our faculties, is insoluble, and in truth a "mystery." Mystery in science (he explains) means the isolation of a fact from all others; when it is explained by a generalizing process, whereby it is shown to come under a general principle or law in common with other isolated facts, the mystery ceases. But when Professor Bain comes to apply this sound view "to the union of mind and body," he becomes obscure through vague use of terms; for "body," as living matter, is used as synonymous with matter which is not living or inorganic, when in truth there is no available comparison between them in their direct relations to mind. It is through the living body, and the changes therein induced, that all the physical agents termed impressions derived from matter act on the mind. Neglecting this fundamental fact, we have discussions as to extension, inertia, gravity, according to the old philosophical method, because living bodies are extended, and have inertia and gravity. Colour is declared to be a truly material property, because it cannot attach to a feeling, properly so called, or a pleasure or a pain. Colour, as a fact, is due to the undulations or vibrations of light, that is, to force or to motion of something impinging on the retina, and thence transmitted to the percipient tissue or organ in the brain; and the differences in colour are probably due to differences in the force and the number of these vibrations in a second of time. Would they, striking thus upon inanimate objects, give results otherwise than chemical? In other words, is not colour a word for the effects of these vibrations on the consciousness or mind acting through the brain? How, this being the fact, can colour and its variations be a property of matter? Are they not due rather to variations in those varying properties of matter which modify the number and kind of vibrations in living matter?

The notions expressed in this chapter clearly show that Professor Bain has yet much to learn as to the part which the molecular forces play, both in exciting the vital activities of the living brain, and in carrying on those infinitely varied activities which are ultimately chemical. The generalizations of this class have not yet reached him, and so he finds "vital properties" to "verge on the mysterious." In this chapter he shows very well how many difficulties, in understanding the science of mind and body, arise from the imperfect meaning and abusive use of abstract terms. It would have been useful if he had shown that every so-called mental state is bodily in a scientific sense, and not troubled himself to explain what is in one sense inexplicable, but in another, and practically, just as explicable as the union of gravity, electricity, chemical affinity, and magnetism with matter. All we need do is to say, that on the one hand is a consciousness of a certain kind, and on the other there are the coincident molecular, yet vital changes.

In his seventh and last chapter Professor Bain considers the "History of the Theories of the Soul." He restricts himself, however, to a notice of the development and decay of "immaterialism," and naturally brings to his aid the researches of Mr Tylor into the origin and development of "Animism." Doubtless, he could adduce good reasons for taking up this polemical question as a question of science; however, truth is best advanced by expounding the truth, and showing practically the best methods for acquiring a higher and higher knowledge. From this point of view it would have been more useful to have given the history of those systems and methods which resemble his own, and are, according to him, growing in public estimation. A critical review of such writers as Hartley, Tucker, Leibnitz, Bonnet, Hall, would have taught Professor Bain that useful lesson, what to avoid. He makes a calculation of the mental and cerebral "acquisitions" attainable by a man on the assumption (altogether gratuitous) that each group of brain-cells represents an "acquisition," and makes out that with 50,000 "acquisitions" there would be for each 20,000 cells and 100,000 fibres. He reckons that there are 1,200,000,000 of cells in the gray covering of the hemispheres, and 4,800,000,000 of fibres, but with an air of moderation assumes the respective numbers to be 1,000,000,000 corpuscles and 5,000,000,000 fibres. Hook calculated how many of these *vestigiu* could be made in a day. Haller was content to say that the number is infinite. If Professor Bain would refer to Charles Bonnet's *Essai Analytique sur les Facultés de l'Ame* (2d edition, 1769), he would see with what precision his own method was carried out by that eminent psychologist; with what little result, however, he, as well as others equally eminent, thus laboured, the history of philosophical progress during the past century tells too plainly. It teaches us that this method fails to satisfy the higher class of thinkers, because it is too imperfect and crude. Even the method of proof of the fundamental fact rightly adopted by Professor Bain hinders its acceptance. He quotes, for example, the facts as to the pathological anatomy of insanity, published by an asylum physician, as conclusive. The forms of disease with, or of, which the patients died, were general paralysis, chronic dementia, epileptic insanity, dementia with paralysis. In these cases nine species of morbid changes were said to have been discovered by microscopical examination as causes of the mental state. This crude materialism, for such it is, only tells the immaterialist what was associated with mental deprivation and death, but nothing of what was going on in the living active brain. That, however, is the problem to solve, and it is only by what the biologist has taught as the laws of life, that the laws of mind will be duly elucidated. It is these laws that have to be compared, and not the microscopical appearances of dead *débris* and used-up tissues, the end of a long series of vital changes which, if we accept even a portion of the doctrines of evolution, began as to every man in a long distant prehistoric age, and have continued through unnumbered and indeed innumerable generations. The microscopy of cerebral cells and fibres is but a corner of the vast domain of biological science in relation to conscious states. If the method we suggest be the right one, all life has its relations to mind. This, indeed, is an old doctrine, as old at least as Aristotle.

We have thus reviewed Professor Bain's little volume frankly and freely, and have only to add, that it is much too small for its subject and objects.

Fourth Annual Report of the State Board of Health of Massachusetts. 1873.

We cannot but think that, in America, they manage sanitary reporting better than we do. There, many, if not all, the States have their Board of Health exercising a general sanitary superintendence over the community in town and country, and with a wide area for observation; their reports, published annually, have an authority and an interest which limited observations of isolated inspectors rarely possess. With such a central sanitary authority, something like method characterizes the action of the medical profession in the prevention of disease and the arrest of epidemics, and opportunities are afforded, by representation to the State Board of Health, for the suggestion of remedies for defects in legislation or in the practice of sanitary medicine. Similar results might be expected in this country were there, let us suppose, a Central Board of Health for the three Lothians—a large district, comprising its city of Edinburgh, its many towns, and more numerous villages and hamlets—whose duty it would be to inquire regularly into the sanitary condition of its varied inhabitants, and from time to time to investigate all causes tending to deteriorate the public health. Our Board of Supervision has, no doubt, full sway in sanitary matters over Scotland, but the area is too extensive; and as the Board is comparatively new to its sanitary duties, while its time is already amply occupied with the supervision of our complicated parochial system, it is not to be wondered at that, as yet, it has achieved little in the way of a general sanitary reform. It has, however—what America apparently has not—a good Sanitary Act to work with. And it is evident that what sanitary reformers are clamouring for on the other side of the Atlantic, is just such an Act as our Public Health (Scotland) Act, 1867, with its stringent provisions regarding epidemic disease. Think of chapter 26 of the General Statutes of Massachusetts providing that "persons affected with *diseases dangerous to public health* may, if in the judgment of the health authorities it be needful, be removed to hospitals specially prepared for their use, or be so guarded in their own dwellings that the public may be safe from contagion;" and section 57 of the same chapter, providing that these laws for the isolation of persons affected with diseases liable to spread by contagion "shall not apply to smallpox!" It need be no matter of astonishment that this loathsome disease has run, and is proving a very fatal curse, throughout the Union. The Local Vaccination Act of Massachusetts manifestly requires amendment, when we find it allowing two years for the vaccination of children. Strong recommendations are urged in the present Report that vaccination should be required in the first six months.

As might have been expected, the questions relating to sewerage and sewage, pollution of streams, and water-supply of towns, come up for discussion, and recent English reports are largely laid under contribution. The "dry-earth" system is found wanting, and we think with reason. The following remarks commend themselves for their common sense:—"The conditions under which the dry-earth plan can be used are exceedingly limited. In the first place, the earth must be dried, and must be kept dry at all times for immediate use. This involves labour and intelligent care and foresight. In the second place, the artificial drying of either earth or coal-ash for use a *second* time in private houses would be expensive, inconsistent with our social arrangements, and almost impracticable. In the third place, no slops or sink-wash, or any other fluids, can be added without causing foul odours and making a nuisance; and from this it is seen that drains and sewers are still absolutely required, and that the dry earth system cannot supplant them, but only relieve them of certain offensive contents. In the fourth place, no one has as yet shown that the foul odour is any measure of the danger from the retention of human excreta about our dwellings. It may be that, while deprived of offensive smell, these materials may yet, under certain circumstances, convey disease. The difficulties which must attend the general use of the dry-earth plan in densely-peopled cities and towns seem to us insuperable. . . . But the case is altogeher different with country houses."

We extract, with approval, the following remarks from a short chapter on that important subject, "The Ventilation of House-Drains":—"The sensible properties of sewer-air are quite remarkable. It is by no means fœtid, as many people suppose, neither is it pungent or ammoniacal. It is rather negative in character, faint in odour, mawkish, smelling, perhaps, more like soap than any other familiar substance. Sewer-air may escape very freely in our dwellings before its presence will be suspected, and that this happens very often there can be no sort of doubt. There are many reasons for this belief. One cause for such escape, and a very active one, is found in the difference of temperature between the interiors of our houses and the interiors of the underground sewers. A rarefaction of air and an upward current are thus induced. The joinings of the soil-pipes are imperfect from alternate expansion and contraction by "exposure to hot and cold water; and, unless a free and safe vent is provided above, there must be leakage at these points."

As to water-supply, our American brethren are meeting the same difficulties that beset us in this country. Streams are undergoing constant contamination; and from the increasing cultivation of the land, springs are becoming less and less available. The following is a good commentary on certain doings in our own metropolis:—"In seeking a remedy for this inevitable difficulty in the future—a remedy which shall be of general application, and which, if made a part of the State policy, would go very far towards furnishing the people with pure drinking-water in all coming time—we find, in rejecting the streams and looking to the *lakes* and *great ponds* as sources of water-supply. Their waters are generally of singular purity, and this is due to their secluded position, to the geological formation of the territory which they drain, and to their action as settling basins for all extraneous matter. They are, indeed, reservoirs provided by nature for the health and refreshment of the people; and we greatly desire that their providential escape from contamination up to the present day should be understood, and that the need of guarding and preserving them in the future for the public benefit should be appreciated."

An interesting inquiry is started in an article on Beershops and Prohibitory Laws, by Mr Aldrich, as to the effect of the use of such comparatively mild liquors as beer and ale in supplanting or preventing the craving of stronger stimulants; and striking evidence is adduced from the past history of such an isolated state as Sweden. The *facilis descensus Averni* is thus divided into stages:—First, *ale* and *mead* were the only drinks; second, *wine*, leading to drunkenness; third, *distilled spirit*, producing such a state of things as to call for the prohibitory act of the government. And the question is put—If the use of ale and beer will cure or prevent the appetite for stronger drinks, why did it not have that effect among the Swedes?

Dr Oliver furnishes an exhaustive report on the character of substances used for flavouring articles of food and drink, in which interesting information is given as to the large quantities that are used of unrectified oil of bitter almonds, of nitro-benzole, and the various artificial fruit essences. These latter substances are not only employed as flavouring ingredients in sweetmeats, but also in fruit jellies and in alcoholic liquors. Amusing details are given of the recipes for the concoction of Irish whisky, New England rum, London sherry and port wine, etc. The flavours and ethers mentioned are sometimes mixtures of two or more of the compound ethers, experience teaching the "druggist" the kinds and the proportions of each kind necessary to produce the taste of the wine or other liquor it is desired to imitate. Cases of poisoning from vanilla are recorded—the extract undergoing a change (probably through oxidation), so as to be capable of producing serious symptoms; and the fact is mentioned, that the low-priced extracts of vanilla in the market are made wholly, or in part, of

the tonka bean—a cheaper article, which is also used for flavouring snuff.

Other interesting papers follow—on the Adulteration of Milk, on the Causes of Consumption, and on the Houses of the Poor in our Cities—to which we may hereafter refer. At present, we conclude by again commending this able series of Reports as worthy of the most enlightened State of the Union, and as calculated to do much for the improvement of the public health.

Traité des Sections Nerveuses. Par E. LETIEVANT, Chirurgien en chef designé de l'Hôtel Dieu de Lyon, etc. Paris: J. B. Baillière et Fils: 1873.

THE author divides his work into three parts: the first being a study of what he terms the pathological physiology of nerve section in man; the second discusses the indications for dividing a nerve in disease; and the third, operative procedure in neurotomy. There is a copious bibliographic index, and table of contents. The book opens with an account of the pathological physiology of the median nerve after section, deductions being drawn from twenty-five cases in which this nerve was either accidentally or intentionally divided.

If within a few hours after section the sensibility of parts to which the nerve is distributed be tested, it will in many instances be found, that though somewhat impaired, it is by no means altogether abolished. And, further, the usual muscular movements of a part may be accomplished more or less perfectly. M. Létiévant argues, with much reason, that when this is the case, in, for example, three hours after section, as in one of the cases recorded, it can scarcely be supposed that a regeneration of the divided nerve has taken place in so short a space of time, and that the sensory and motor phenomena are direct. Nor can he admit, in explanation of these facts, an incomplete division of the nerve.

There remain the theories of substitute sensibility and motion (*sensibilité supplèce et motilité supplèce*), by means of the neighbouring nerves.

This sensibility by substitution (*par suppléance*) is, according to M. Létiévant, accomplished by collateral channels, establishing a connexion between the insensible surfaces and the nerve centres, these channels being (1) nervous anastomoses, and (2) normal papillæ in the neighbourhood of a paralyzed region. To prove the former, the author adduces anatomical facts, pathological observations, and experimental phenomena.

Case III. recorded is an interesting example of the second of these experiments. Division of the median had taken place above the wrist. Irritation applied to the nerve above the seat of division produced no effect; but, at or below that point, was followed by a characteristic prickling sensation. MM. Arloing and Tripier divided successively three collateral nerves in the toe of a dog, yet sensibility persisted at all points. They then divided the fourth and last, when insensibility to pain became absolute. This experiment is given as an incontestible example of sensibility by anastomotic substitution. The subject of Case V. led M. Létiévant to believe that the papillæ in the neighbourhood of a paralyzed region played a part in this compensatory sensibility. Vainly did he endeavour to produce a sense of pain by plunging a pin even to the bone, in the end of the index finger, and by immersing that finger in water at a temperature which the other fingers could not bear for a moment; yet on scratching the end of the finger with the point of a pin, not only was this felt, but also the seat, intensity, and direction of the scratching. Further, M. Létiévant adds, "I took between my index and middle fingers, the finger of M. X., and turned away my head while concentrating my attention. I asked M. X. to rub (or scratch) *his own* finger. These rubbings I felt and knew their site, intensity, and direction." M. X. then pricked his own finger, and naturally felt it, while M. Létiévant did not. "By what physiological mechanism," asks M. Létiévant, "did I perceive these scratching sensations which M. X. exercised on his own finger?" He explains it by the "*retentissement*" which this scratching produced on his (M. L.'s) papillæ. These, though distant from the point touched by the pin, perceived an impression which, transmitted to the brain, is immediately attributed to distant scratching. "It is a sensation analogous to that which a surgeon feels when he encounters, by means of a probe, rugosities or resistance in a wound."

It will be admitted by all as most interesting, that the patient in Case V. should have felt a slight scratch, while he remained altogether unconscious of a severe prick; but we doubt whether M. Létiévant's explanation will be universally accepted. We have ourselves repeated M. L.'s experiment, and have had an equal perception of this mediate scratching. The act of scratching, say with the point of a pin, as in M. L.'s case, however gently practised, must produce a temporary though slight depression in the finger scratched, at the point scratched. This implies a displacement of the substance of the finger outwards from the depression, which signifies an impulse, however slight, against the papillæ of the embracing finger, followed in the usual manner by a perception of that impulse.

We are somewhat surprised to find that M. Létiévant does not refer to the researches of Szymanowski (*Prag. Vjschr.*, 1865, IV. p. 52, and quoted in Uhle and Wagner's *Pathologie*), who proved this transmission of nervous influences by means of anastomotic branches, (1) by anatomical preparations, (2) by the sensibility to pain which the patient has sometimes evinced even after complete section of the central cord of the nerve, when the peripheral portion was cut into;

and (3) from the fact, that sensation is seldom at once and completely abolished by section of the nerves of the face. That the movements of a part after section of a given nerve are not dependent on the muscles supplied by that nerve, M. Létiévant concludes, from the fact that the muscular portion is not felt to contract, nor do the tendons stand out under the integument in a cord-like form. Further, after a time the muscles supplied by the divided nerve atrophy, and other muscles with a normal nerve-supply contracting, give rise to forms of deformity which are even diagnostic of the nerve divided.

As time elapses sensibility and motion improve, until they are completely re-established. This, however, being a consequence of the regeneration of the nerve, whose solution of continuity no longer exists.

Just as M. L. has a *sensibilité supplée*, so has he a *motilité supplée*. These various phases were well illustrated by a patient whose median nerve M. L. divided for tetanus following a crush of the hand on 22d December 1867, who still had, on 25th October 1868, substitute motion and sensation well marked and demonstrated in the hand and fingers. In the fourteenth month, the return to a normal state made a rapid stride; and, finally, in the nineteenth month (20th July 1869), the resumption of functions was almost complete, the nerve having been evidently regenerated.

In many of the cases recorded, ulcerations and other evidences of impaired vitality were exhibited by the hand and fingers after division of the nerve; a result which M. Létiévant attributes in the accidental cases as much to injury of the soft parts and blood-vessels, as to the nerve lesion; while he, at the same time, by no means overlooks the trophic influence of the nerve. M. L. mentions the cases of accidental division of the median referred to by Paget. A third case, described by the same author, though not one of division of the nerve, exemplified in a marked degree the trophic influence of such a nerve as the median, independently of blood-vessel lesion. We refer to the case of fracture of the lower end of the radius, which, being repaired by an excess of new bone pressing upon the median, was followed by ulceration of the thumb, fore and middle fingers, which only disappeared when the wrist was so bound, "that the parts on the palmar aspect being relaxed, the pressure on the nerve was removed," the ulcers again breaking out when the patient was allowed to use his hand freely, and the pressure on the nerve was renewed. . . . .

The other subjects treated of in this part are the consequences of section of the ulnar and radial nerves, of the simultaneous section of several important nerves of the upper extremity, of section of the nerves of the lower extremity, and, lastly, of the nerves of the face.

In the second part the author discusses the indications for nerve section in neuralgia, for the pain of cancer and ulcers, for traumatic tetanus, for epilepsy, one case of section of the internal plantar nerve in a boy aged thirteen, who was affected with chorea, evidently from a neuroma of that nerve. Removal of the tumour was followed by a permanent cure. Chorea, epilepsy, tetanus, and neuralgia are all considered suitable cases for neurotomy, when the exciting cause seems to be peripheral irritation. M. Létiévant has also a chapter on removal of tumours of nerves, and another, finally, on the old and new methods of nerve section, with an appendix on neurotomy for phlegmonous inflammation and glaucoma.

In the third and concluding part, treating of operative procedure, we have concise anatomical descriptions, with superficial guides to the nerves, and other points of interest. This part is copiously and well illustrated, and the whole book is clearly printed on good paper. The entire work is to be considered a very complete, carefully-compiled, and well-thought-out treatise on nerve section, which will well repay perusal.

Clinical Researches in Electro-Surgery. By A. D. ROCKWELL, A.M., M.D., etc.; and GEO. M. BEARD, A.M., M.D., etc. New York: William Wood and Co.: 1873.

DRS ROCKWELL and BEARD, Fellows of the N.Y. Academy of Medicine, and electro-therapeutists to certain medical institutions in New York, have written a book on electro-surgery. It is difficult to discover their object for doing so. Suffering humanity will not be benefited thereby, their fellow-practitioners will not be instructed. It seems intended to intimate that Drs Rockwell and Beard are prepared to apply some form of electricity to any surgical sufferer that may fall into their hands.

There are three modes in which electricity may be applied in surgical practice. By passing electric currents of one kind or other through the body we may seek to modify nerve-action and nutrition; by electrolysis we may decompose the tissues; by the galvanic cautery we may burn them. The first method has been usefully employed to relieve certain kinds of pain, to stimulate partially-paralyzed muscles, to resolve chronic inflammatory thickenings; and, although precision and definition are yet to be acquired, the general principles by which the practitioner must be guided are tolerably well understood.

The clinical researches of Drs Rockwell and Beard have, however, been mainly directed to the treatment of skin diseases. They have devised a method of "central galvanization," whereby the negative pole is applied to the epigastrium, while the positive so wanders over the body as to bring every portion of the central nervous system within the action of the current. To this application all skin diseases succumb,—eczema, prurigo, acne, lichen ægrius, pityriasis, psoriasis, elephantiasis, herpes zoster, and even herpes circinatus; at least, according to them, if localized electrization fail, central electrization will succeed, or *vice versa*.

It is now established that electrolysis is an admirable method of curing cirsoid aneurism, that it is worthy of further trial in certain kinds of aneurism proper, that many large nævi are best treated by that means, and that a few other tumours, mainly perhaps the hyperplastic, may thus justifiably be attacked. But it seems also well established that to electrolyze malignant growths is, in other words, to hasten their spread and ulceration, and that the electrolysis of most simple tumours is so tedious, troublesome, and uncertain, that if no other mode of treatment be possible, they are better left alone.

Drs Rockwell and Beard despise precedent. They apply electrolysis not only to vascular growths and goîtres, in which success may be legitimately expected, but also to cysts, fibroids, recurrent fibroids, epithelioma, scirrhus. Their cases are not encouraging, but they are exceedingly jubilant over an invention of their own which they call "working up the base." This consists in so electrolyzing the circumference of a tumour as to cause it to slough out—a proceeding from all points of view quite unjustifiable; for assuredly, if enucleation by the knife be unadvisable, the galvanic cautery is infinitely to be preferred to electrolysis.

They do not appear to have largely used the galvanic cautery. In this country we know it as a most safe and convenient mode of removing growths from parts where ligature is difficult of application. They record at immense length, and as a new era in surgery, its partial use in a case of lupoid or epithelial disease of the vulva—a disease for which, in the Edinburgh Infirmary, the operation has been several times most successfully performed without flourish of trumpets.

In short, this clinical research is a production of little value. The descriptions of the cases are inaccurate and indefinite, the methods of treatment are inappropriate, and the conclusions are ludicrous or unproved.

Diseases and Injuries of the Eye: Their Medical and Surgical Treatment. By GEORGE LAWSON, F.R.C.S., Surgeon to the Royal London Ophthalmic Hospital, Moorfields, etc. Second Edition. With 88 Wood Engravings. London: Henry Renshaw: 1874.

THIS is one of the series of exceedingly neat, handy little manuals in process of publication by Mr Renshaw; and which, having already attained a second edition, seems to have met with a rapid success. Mr Lawson has the qualifications for writing such a manual; he knows his subject, has large opportunities for keeping up his experience, and, what is of still greater importance, has the power of expressing his opinions in simple vigorous English, without redundancy, pedantry, or repetition. He has succeeded in furnishing the student or practitioner with a simple, sensible, moderately brief account of all the more common injuries and diseases of the eye and its appendages.

The descriptions are vivid and life-like; the arrangement is good and simple; so that, by the full index, any subject can be at once got at. The treatment is up to date; the labours of others are fully and frankly acknowledged, and there is no undue egotism or exhibition of idiosyncrasies; in a word, we have rarely seen a work of the kind which so thoroughly fulfils its purpose, in which there is so little to blame and so much to praise.

Mr Lawson's industry must be great, as he is not only surgeon to the special Hospital at Moorfields, but also is full surgeon in the general Wards of the Middlesex Hospital.

Traité de Chirurgie d'Armée. Par L. LEGOUEST, Inspecteur du Service de Santé de l'Armée, etc. Deuxième Edition, revue et augmentée. Paris: Baillière et Fils: 1872. Pp. 800.

NUMEROUS works have been issued since the late Franco-German war, which treat of isolated points in its surgical history; but until the appearance of the book now before us, no attempt, as far as we are aware, has been made to exhibit, in a united whole, the many modifications and improvements military surgery has undergone in that campaign. Mons. Legouest, surgeon-in-chief to the 1st corps of the Army of the Rhine, in bringing out this the second edition of his work on Military Surgery, has given the results of his own observations, and has utilized the works of other surgeons engaged in the war.

An idea of the comprehensive character of this treatise may be gathered from an epitome of its contents:—

In Chapter I. the different weapons of warfare, and their modes of action, are clearly described. Chapters II., III., and IV. treat of punctured and incised wounds, and of contusions. Chapter III. contains a detailed description of the operation for ligature of all the more important arteries.

Chapters V., VI., and VII. are devoted to the subject of gunshot wounds. Chapters VIII. to XV. enter fully into wounds of the head, neck, thorax, abdomen and pelvis, and extremities. Each region is considered separately. The surgery of fractures is very complete. Clear descriptions are given of the different forms of splints, their application and construction. Chapter XVI. treats of amputations and resections. The influence of atmospheric changes on the health of troops is considered in Chapter XVII. Chapter XVIII. is devoted to a consideration of tetanus, pyæmia, septicæmia, and hospital gangrene. Mons. Legouest's views on the relationship between "l'infection purulente" and "l'infection putride" are well worth perusal. Chapter XIX. describes the results obtained by surgical operations. In Chapter XX. detailed descriptions are given of ambulance waggons, their construction and equipment, and of other means of transporting the sick and wounded. The construction of field hospitals is clearly described.

From the above short epitome, the comprehensive and practical character of this work will at once be seen. The illustrations are clear and distinct, and care is always taken to give precise references to any authorities quoted.

"Nous ne nous sommes point écarté du caractère pratique que tout d'abord nous avons voulu donner à notre ouvrage, et nous pouvons dire de nouveau; sobre dans l'exposé des théories nous avons formulé les préceptes dont l'observation a démontré l'excellence; notre but a été de donner aux jeunes médecins militaires un guide propre à diriger leurs premiers pas dans la carrière, et aux praticiens un livre qui leur permît de rappeler et de préciser leurs souvenirs." Such is the last paragraph in Mons. Legouest's preface. We believe that he has attained his object, and therefore recommend his book to the notice of English military surgeons.

A Handbook of the Theory and Practice of Medicine. By FREDERICK T. ROBERTS, M.D., etc. London: H. K. Lewis: 1873. Pp. 1043.

THIS is a good book, yea, a very good book, judging by the standard at which it aims,—a simple grinding book for students. Within the compass of its pages there is no better book of a similar kind; it is not so scientific in its pathology—or rather, perhaps, it would be more accurate to say, that it is not so full in its pathology—as Aitken, not so charming in its composition as Watson, not so decisive in its treatment as Tanner; but it is more compendious than any of these, and therefore more useful, perhaps, for the early days of a student, when he wants most of all mere heads of lectures, notes of pathology, and variety of treatment, which he may fill up, annotate, and emphasize, according to the tuition of his teachers, and subsequently correct from his own experience. There is one other point in which this work is, we think, superior to all those of the present day which it attempts to emulate, and that is in the definite way in which it connects its own statements with the facts obtained by clinical examination; thus it contains within itself the elements of its own amendment, and the germs of future progress in the science and art of medicine. Dr Roberts first of all gives a general introduction to the mode of case-taking, which differs in no essential particular from those generally in use, and then, before entering upon the diseases of any special organ, he also gives a summary of the clinical phenomena, including the symptoms and physical signs presented by each organ under varying circumstances. Now, for the student, nothing can be of greater value: the important connexion which apparently trifling facts often have with the etiology of the disease can only be fully appreciated when these have been carefully elucidated, summarized, and brought into their proper relation with the symptoms developed. A *magister artis* can usually without any difficulty, or any minute inquiry, make a rational diagnosis of most diseases, and propound a reasonable treatment; but it is often astonishing even to him how much light can be thrown, not upon the diagnosis, but upon the etiology of the disease, by some trifling fact only to be ascertained by a most careful and painstaking investigation. In this important respect we know of no work in the English language—or any other, for that matter—which competes with this one. There is no difficulty in finding in this book matter for disagreement, or for captious criticism; we prefer to point out its peculiar germ of truth, and to express our hope that students may give free scope for the growth of so important an object, and that teachers may on their part fill up and correct the outlines of etiology, pathology, and treatment which it proffers.

A Manual of the Practice of Surgery. By W. FAIRLIE CLARKE, M.A. and M.B. Oxon., F.R.C.S.; Assistant-Surgeon to Charing Cross Hospital. Second Edition, revised and enlarged. 144 Wood Engravings. Pp. 402. London: Henry Renshaw: 1874.

As in these pages (July 1865) we reviewed the first edition of this work on its appearance, we need now only allude to the changes and additions which are certainly improvements in the present edition. It is now one of those exceedingly neatly got-up and well-printed manuals which Mr Renshaw is bringing out. The letterpress is carefully revised and brought down to the present date; and though it is still to our mind too short and bald a work to be really instructive to a good student, its very brevity and simplicity make it easily read, and readily "ground up" by a lazy one. It is not sufficiently full, however, to be trusted to, even for a pass examination. Lithotomy, for example, is treated of in two pages; and the suprapubic operation is only named, not described. The chapter on Operations is still far from satisfactory, either in selection or description. For example, Syme's operation is very incorrectly described.

The woodcuts have the great and singular merit of being nearly all original, making a most charming variety from the old stock cuts with which most of our Systems of Surgery are ornamented. Some of them, as Fig. 9, Fig. 47, are neither instructive nor artistic.

The table of formulæ and receipts will prove useful to the student, and tends to conciseness, as references to it prevent undue repetition in the text.

We believe that Mr Clarke has succeeded admirably in what he has attempted: to produce an easily-read and short little text-book for students. Whether such works are good for the students and for science, is another and much more difficult question.

Don Carlos: A Tragedy. By SCHILLER. Translated into English Blank Verse, by ANDREW WOOD, M.D., F.R.S.E. Edinburgh: W. P. Nimmo.

THE comparatively late period at which this work has been received for review, and the press of matter already on hand, necessitate a longer notice of it being postponed till next month. It will then, however, afford us much pleasure to call the attention of our readers to this able translation—which, emanating as it does from the pen of one of the most active and popular of the medical profession, possesses attractions of no ordinary interest and importance.

Veterinary Medicines: Their Action and Uses. By FINLAY DUN, formerly Lecturer on Materia Medica and Dietetics at the Edinburgh Veterinary College. Fourth Edition. Edinburgh: Edmonston and Douglas: 1874.

WE have just received the fourth edition of this truly admirable work. Every article has been most carefully re-written, and the new chemical nomenclature and notation adopted.

Mr Dun has added in his best style the most recent investigations in veterinary materia medica; but, we observe, has most wisely avoided even noticing the many errors which are abroad. We believe this book to be by far the best work on the subject in any language we have yet seen; alike invaluable to every veterinary surgeon, student, and agriculturist.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING II.

Wednesday, 3*d December* 1873.—Dr G. W. BALFOUR, *and, for a time*, Dr ARGYLL ROBERTSON, *in the Chair.*

Exhibition of PATHOLOGICAL SPECIMENS:—

I. *Dr Argyll Robertson* exhibited an EYEBALL, from which projected a pedunculated tumour, about two inches in diameter. The patient from whom the eyeball had been removed was a woman about 50 years of age, apparently in excellent health. The eye had, for above twenty years, been blind and disorganized, the result of previous inflammation, but no projection or growth from it was observed till five months ago, when a small yellowish white spot was observed on the sclerotic, close to the inferior margin of the cornea. This rapidly increased in size, projected forwards through the palpebral aperture, and spread out, covering the external surface of the lids. As, on examination, the movements of the eye were not materially impaired, and the eye not bulged forwards, Dr Robertson concluded that the disease was confined to the eyeball, and did not involve the optic nerve or cellular contents of the orbit, and therefore had recourse to simple enucleation of the globe with the attached growth. After enucleation, the contents of the orbit appeared free from disease; but a week after the operation, two small nodules appeared in the conjunctival cicatrix, which in two days increased much in size, while a third nodule was indistinctly felt deep in the orbit, indicating a return of the disease. This necessitated the removal of the whole contents of the orbit, from which operation the patient is at present recovering. The tumour, on examination, was found to be a soft spindle-celled sarcoma. One peculiarity of the growth was that, while it had projected so markedly forwards, it had only slightly involved the coats of the eyeball, and only protruded to a minute extent into the chamber of the vitreous humour.

II. *Mr Annandale* showed—(1.) A FIBRO-CELLULAR TUMOUR, the size of an orange, which he had successfully removed from the region of the throat and left tonsil, by cutting through the left angle of the lower jaw, and exposing the tumour by a dissection of the pterygoid space.

(2.) A NEUROMA, the size of a small walnut, removed from the axilla.

(3.) A large GLANDULAR TUMOUR, removed from the female breast.

(4.) A large LYMPHOMA, removed from the neck of a patient. In order to expose the tumour, the sterno-mastoid muscle was divided, and its ends turned aside, at the operation.

(5.) An example of EPITHELIAL CANCER, including the penis, scrotum, and skin over the pubes. The whole of the diseased parts, together with both testicles, was successfully removed, and the patient had made an excellent recovery.

(6.) The BREAST-BONE OF A PARTRIDGE, which he had successfully removed from the œsophagus of a patient. The bone had been swallowed the previous day, and was firmly impacted in the canal.

III. *Dr James Carmichael* exhibited two GALL-STONES, recently passed, per anum, by an elderly lady. The larger stone was of conical shape and considerable size, weighing 164 grains; the smaller, cup-shaped, weighed only 30 grains. On examination, they were found to be composed chiefly of cholesterine. The patient's illness extended over about a fortnight; the symptoms, which were of a dyspeptic character, not being well marked, consisted chiefly of nausea, epigastric tenderness, flatulence and constipation, and a peculiar feeling at the pit of the stomach, as if "there was a beast there." Throughout the case, there was no pain of great severity, and no jaundice. The question of how a stone of so large a size could pass into the intestines was interesting. One of two ways occurred to him, either that the calculus had passed the duct by a process of gradual dilatation, as in case recorded by the late Dr Cullen, where the tube was enormously dilated so as to admit of several fingers; or that it had passed through a fistulous opening between the gall-bladder and duodenum; the latter being a possible explanation in this case, judging from the character of the symptoms, the absence of pain of great severity and jaundice negativing the idea of the calculus passing through the ducts.

IV. *Dr Cuthbert* showed a small CALCULUS, which he had recently removed from the point of the urethra of a child who had suffered from retention of urine for twenty-four hours, for the relief of which the mother had been exhibiting sweet spirit of nitre. A large quantity of urine was passed immediately.

Dr G. W. Balfour then left the chair, and exhibited the preparations of morbid structures referred to in the following remarks:—

V. *Dr George W. Balfour* exhibited the morbid parts connected with TWO CASES OF ANEURISM, the subjects of which had both been repeatedly exhibited to the Society, while alive, as examples of the benefits to be derived from the use in such cases of large doses of the iodide of potassium. One of these patients, Peter Rice, had been under treatment for over five years; the other, James Wilson, for over six years; both of these patients had been very seriously ill when they first came under treatment, and both had received so much benefit from the treatment as to be restored to comparative health and strength, and enabled to obtain their own livelihood by manual labour, in the one case continued up to the moment of death, in the other up to within a few weeks of that event. The members of the Society had repeated opportunities of ascertaining for themselves the extent and reality of the great relief which these patients had obtained, and the cases of both of them had also been fully recorded, and these were now supplemented by the post-mortem history, which confirmed in a very remarkable manner a theory of the way in which the relief obtained was produced, which he (Dr B.) had for some years looked upon as being most probable. Rice died suddenly in the summer of 1872, while at work in the Botanic Gardens here; he had a bad cough for a week or two previously, and the immediate cause of death was hæmorrhage into the pericardium, the source of which was a very minute rent at a comparatively thin part of the aneurism just where it sprang from the aorta. The aneurism itself was the size of a small cocoa-nut, everywhere dense and firm-walled, except just where the rupture had taken plaçe; it contained only post-mortem clots. Wilson died a few weeks ago in Ward V. of the Royal Infirmary, where he had been for many weeks in broken health, weak-minded, and childish; he died frem subperitoneal hæmorrhage, the result of rupture of an abdominal aneurism; from the symptoms the hæmorrhage was probably gradual. This man had originally an aneurism implicating the innominate, subclavian, and carotid arteries, forming a pulsating swelling in the neck as large as a good-sized kidney potato; under treatment this ceased to swell up or present any of the characteristics of an aneurism, but the arteries remained always somewhat larger than normal; the brachial pulse on the left side was almost imperceptible, whilst the radial pulse never could be felt. He also had originally an aneurism of the abdominal aorta the size of a small orange, situated in the lower part of the epigastric region; under treatment this tumour gradually ceased to present any of the characteristics of an aneurism, and could only be felt as a hard firm knot, apparently cured by fibrinous deposit within the sac. Three years ago he had a recurrence of his aneurismal symptoms, with a large pulsating tumour above the old hard knot; after a time this also became firm, was contracting, and bid fair to become as innocuous as the old one. When he returned to the Infirmary several weeks ago, complaining of the symptoms which continued till his death, the large abdominal aneurism, occupying almost the whole of the left hypochondriac region, was at first fluid, but seemed gradually to solidify under the influence of rest and treatment, but without any diminution in its size. After death the whole aortic system was found to be greatly atheromatous, innominate, right subclavian, and carotid arteries were thickened, and seemed somewhat larger than usual. The left subclavian was blocked by a firm fibrinous clot just where it passed out of the chest, the coats of the artery being contracted round it. The descending aorta gradually dilated, till, after passing through the diaphragm, it developed into an aneurism the size of a large cocoa-nut, beneath which could be felt the firm hard nodule of the original aneurism. This case, during life, was thought to show that aneurism might become cured in two ways: first, by the contraction and condensation of its walls, so that it became simply a large dilated artery, still pervious; and, second, by the formation of clots within the sac of the aneurism, and their gradual condensation and contraction. The post-mortem appearances of Rice and of Wilson, so far as his innominate aneurism was concerned, proved that relief was certainly occasionally obtained in the first method; while the life-history of Rice and of Wilson's abdominal aneurisms was held to prove the occasional occurrence of relief from the second mode; but on examining the aneurisms after death, they were indeed found filled with clots, some of which were of some standing, but none were decolorized or fibrinous, while the hard nodule of Wilson's original aneurism was found to be owing to a calcareous plate developed in its walls. The post-mortem history of all of these aneurisms, therefore, confirms the view that relief is obtained, not by clot-formation, but by the contraction and condensation of the sac, and that if coagulation takes place within it at all, it plays a comparatively unimportant part, depends upon the remora of the blood, and seems in no respect to be due to the actiom of the iodide of potassium. Dr Balfour had seen one remarkable instance confirmatory of this fact, which also seemed to have an important bearing on the treatment of aneurism by compression. In this case the man laboured under a large and very tense innominate aneurism projecting into the neck; he was just beginning to feel the good effects of the iodide when it suddenly ruptured, apparently into the outer coat; a large livid pulsating tumour the size of a child's head was formed in the neck, and lay beneath the man's chin, while a boggy pulsation could also be felt between the two first intercostal spaces. Fearing external rupture, Dr Balfour sent for his colleague, Dr John Duncan, to have galvano-puncture performed to prevent this catastrophe; by the time Dr Duncan arrived all pulsation in the tumour had ceased, and at his advice nothing was done beyond continuing the iodide. By degrees the whole of the effused blood, which remained fluid up to the last teaspoonful for a period of about eight weeks, was completely absorbed; by this time all trace of the aneurism had disappeared, and the man was discharged. Some weeks afterwards he returned for inspection, appearing in admirable health, and without any return of his aneurismal symptoms. He subsequently took the command of a small vessel which belonged to himself, and he died suddenly in the course of last summer, probably from another aneurism, the arterial tissue in all such cases being very much diseased. These cases are all very instructive, and go to show, as far as anything can do so, that the remedial influence of the iodide of potassium is not produced by coagulating the blood within the sac, but first of all by its sedative action on the heart, which greatly diminishes the distensile force within the sac, thus permitting it to contract; and, second, by some peculiar action on the fibrous tissue, by which the contraction of the sac is aided, and its walls are strengthened and condensed. In his earliest paper on this mode of treating aneurism, Dr Balfour pointed out that the iodide of potassium could never be regarded as a perfect cure for aneurism, except under the most exceptional circumstances, but that it was a palliative which could be safely employed even in the most hopeless cases, while even in them it relieved suffering and prolonged life. These results were of great importance, and were unquestionably due to the iodide itself, though their production might be aided hy adjuvant treatment. A very interesting case had occurred to him lately, which showed this very clearly: the patient had been for some time under a different treatment without relief; his main symptoms were a remarkable vox anserina, with compression of the veins coming from the head, so that he had constant headache, flushed face, etc.,—all the symptoms of cerebral congestion; under the use of the iodide in twenty-grain doses, without other treatment, all these symptoms were so much relieved, and even his voice so much improved, that the last time he was heard of he had almost ceased to think himself ill.

VI. *Abstract of Paper by Dr T. J. Maclagan.*

Applying his theory of the mode of production of the phenomena of idiopathic fever, to the explanation of the nervous symptoms, the author maintained that these were all of anæmic rather than of uræmic origin.

Rigors he attributed to contraction of the minute arteries of the skin; the headache which accompanies the rigors to anæmia of the brain, the result of a similar contraction of the minute cerebral arteries; the contraction of these vessels being due to an effort to stay the too rapid flow of blood through the capillaries which results from the increased demand for that fluid in the tissues—a demand which is due to the propagation in the tissues of an organism having wants identical in the main with those of the tissues in which it grows.

The contraction of the minute arteries thus induced gives place to relaxation when the disease is fully developed. The delirium which replaces the headache at this stage also results from anæmia, due to the consumption by the contagium of the nutritive elements of the blood.

Convulsions and coma are due to an exaggeration of the same cause.

The author admitted the existence of excess of urea in the blood in such cases, but denied the competence of urea to cause the nervous symptoms. He argued that all were due to anæmia, the result either of contraction of the minute cerebral arteries, or of impoverishment of the blood consequent on the consumption by the contagium of the oxygen, nitrogen, and water, requisite to the nutrition of the tissues. He maintained that there was not sufficient evidence to show that either urea or carbonate of ammonia was capable of giving rise to the symptoms usually attributed to their action. The occurrence of renal complications he regarded as the result, and not the cause, of uræmia, the local renal changes being due to the irritant action on the kidneys of an excess of their natural stimulant.

Even in chronic renal disease it was argued that the head symptoms were due, not to uræmia, but to anæmia. Contraction, and even hypertrophy, of the muscular coat of the minute arteries being a recognised pathological phenomenon in such cases, it was reasoned, that in the presence of so potent a cause of nervous symptoms, we might safely discard one so doubtful as uræmia, notwithstanding its very general acceptance. The greater prominence of the hypertrophy of the muscular coat of the minute arteries in the kidneys was attributed to the fact that these organs would naturally suffer more than any other from the circulation through them of blood charged with excess of urea.

Dr George W. Balfour said that the Society was very much indebted to Dr Maclagan for his most interesting though somewhat speculative paper; and though they were also much obliged and gratified by the manner in which Dr Muirhead, their accomplished secretary, had read the paper, they could not but regret that Dr Maclagan was not there himself to defend his own views, which, so far as they related to fever, were contrary to received opinions, and both in relation to fever and albuminuria were opposed to well-known facts. In the first place, the theory of a living contagium which preyed upon the vitals of the system was merely a modern version of the personification theory of disease, and appeared to Dr Balfour at once unscientific, retrogressive, and unnecessary, except for the purpose to which Dr Maclagan had applied it; for which, moreover, its theoretical and unsupported character rendered it only of doubtful utility. The anæmia theory of uræmia, so far as regards that consequent on albuminuria, was no novelty. It had been first propounded by Dr Owen Rees, and improved upon by Traube. Traube believed that the watery blood of albuminuria favoured serous exudation and cerebral œdema; that this was still further increased by the internal blood-pressure produced by the action of the hypertrophic left ventricle usually present in such cases, while, in its turn, the cerebral œdema compressed the capillaries of the brain, and thus produced cerebral anæmia, which resulted in uræmic coma, when the hemispheres were chiefly involved, and in uræmic convulsions when the ganglia were mainly affected. But the anæmic theory of uræmia was contradicted by the good results formerly obtained by bloodletting in such cases, and by the good results still obtained by bloodletting in puerperal uræmic patients, as well as by the morbid anatomy and pathology of such patients, in which neither anæmia nor any morbid contagium played any part. The experiments of Oppler and Zalesky, etc., had apparently disproved both the ammonia and urea theory of uræmia; and the present standpoint of the profession seemed to be that this depended upon the accumulation in the blood and in the tissues of the products of retrograde metamorphosis which ought to be secreted by the kidneys. This was in fact what the word uræmia actually signified—*ουρον, αιμα*—and it was an unlearned error to suppose that the first part of the word uræmia was meant to represent urea, which was Latin if anything, while the last part was unquestionably of Greek origin. Dr Balfour pointed out that the mere presence in the blood of these products of retrograde metamorphosis was only of consequence in so far as that prevented the healthy nutrition of the tissues. Albuminuric patients often had their blood for long almost saturated with the products of retrograde metamorphosis without any very definite symptoms. The kidneys passed off some of this morbid matter at one end of the vascular connexion, while an equivalent amount was taken in at the other; and thus life was maintained in an oscillating state of uncertainty, exactly as in cardiac disease, where a systole constantly threatened, and might be precipitated at any moment by any accidental disturbance. So it was in kidney disease. These organs might be just working sufficiently to preserve life in a moderate way, when some accidental chill, or some trifling operation, lithotrity—as in the case of the late Emperor of the French—removal of the elongated lip of the os uteri, or of an enlarged tonsil, as he himself had seen, set up slight febrile irritation and increased metamorphosis, the results of which the disorganized kidneys were unable to remove; hence tissue accumulation, *i.e.*, more or less rapid arrest of necessary vital changes, and more or less sudden uræmic sinking, coma, or convulsions. The differences between uræmic sinking, uræmic coma, and uræmic convulsions, depended of course upon the special parts of the cerebro-spinal centres which for the time happened to be most involved in this want of vital renewal. A careful investigation of such cases would ultimately unravel their intricacies, which were only apparent; but it appeared to Dr Balfour most important to remember that the state of the blood was only one link in the chain of causation of such affections, the pivot upon which the whole turned being the condition of the tissues comprising the cerebro-spinal centres; and he believed that the condition of these tissues in lead-poisoning, and the various affections to which that gave rise, afforded a very interesting illustration of the mode in which so-called uræmic poisoning was produced, and the various ways in which it acted.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING I.

12th November 1873.—Dr THOMSON, *President*, in the Chair.

I. CASE OF ECLAMPSIA. BY JOHN BOYD, M.D., SLAMANNAN. COMMUNICATED BY J. JAMIESON, L.R.C.P. AND S. ED.

On the 4th January 1873, I was called, near midnight, to Mrs J., primipara, aged 26, who had been under my care about two months previously for a severe attack of crural phlebitis, the veins of the right limb being highly varicose, which terminated favourably under the usual active antiphlogistic treatment and regimen appropriate to such cases. On my arrival I found the pains regular and pretty strong, the os uteri dilated to about an inch in diameter, the waters broken, and the head presenting in the first position. At two A.M. I was called hastily to the bedside, and found her in violent convulsions. I immediately procured chloroform and the forceps; anæsthetized her, and the fit subsided. An interval of perfect consciousness followed, during which micturition was attended to. The urine was highly albuminous; specific density unascertained. After a time the recurrence of the convulsions necessitated the re-application of chloroform, and the os being now dilated to three inches, I resolved to apply the forceps. The head retreated above the brim so readily before the entering blade, that I inferred the funis being wound round the neck of the infant. I changed Simpson's forceps for a straight and smaller pair which took less room, and at eight A.M. succeeded in extracting a living full-sized male child, round whose neck the cord was twice encircled, the placenta following immediately afterwards.

From an hour subsequent to the delivery, the convulsions continued at half-hour intervals; the patient was comatose, the face dusky and congested, the pulse varying from 80 to 104, the temperature not much elevated, and the skin moist. I caused the hair to be removed and cold water cloths applied to the head, and continued the application of chloroform in moderate doses for two hours longer; but seeing the regular and gravescent occurrence of the fits, I performed venesection to the extent of twelve or fourteen ounces, but from it being inadvertently thrown out while I was binding up the arm, I was unable to ascertain the character or the exact amount of the blood drawn. I next put on the back of the tongue a cathartic powder, consisting of four grains of calomel and six of rhubarb, which was swallowed by aid of several teaspoonfuls of water introduced *seriatim.*

8 P.M.—The patient appears better, but still unconscious. Pulse 80, and firm. Respiration tranquil; temperature 100. The bowels not having been opened, a castor-oil and turpentine enema was thrown up, which brought away a profuse evacuation of normal

colour and consistence, the distended bladder emptying itself at the same time.

5th January.—Passed a quiet night, and had only two or three slight convulsions since last seen; the aspect, pulse, and respiration still favourable. Was unconscious all day, but became awake in the evening, and rather surprised at finding herself denuded of hair and attained to maternity.

From this time forth her progress was rapid and satisfactory; no convulsions nor any other malady apparent. Was able to leave her bed in about a week afterwards, and since then her health has been excellent.

Such is a brief account of a sufficiently ordinary case of eclampsia, which must always be interesting to the practitioner, from the formidable danger and the striking character of the attendant phenomena.

A most important element of disturbance of the circulatory and nervous systems has been discovered in the presence of albuminuria in such affections, indicating serious derangement of the renal functions and imperfect blood-depuration. How long this may have been going on in our present subject it is impossible to guess; her general health seemed very good immediately before her confinement, and on investigation I could make out no evidence of tumefaction of face or upper extremities. The swelling of right foot, from previous attack of phlegmasia dolens and permanent varix, the result seemingly of accidental injury in girlhood, does not appear to bear on the present topic of inquiry. With regard to treatment, there can be little doubt as to the propriety of completing the labour as early as was compatible with non-injury of the maternal tissues. This was decidedly aided by the continuous application of moderate doses of chloroform—the anæsthesia produced by which has been advanced by many as a complete specific for eclampsia, superseding every other curative agency. The importance of the case now related consists, in my opinion, in the fact that, in it, chloroform had no efficacy on the convulsions whatever; that the fits continued from the commencement at half-hour intervals, except for the first hour immediately after delivery, till venesection was adopted; and on looking back at the course of events, I am disposed to take blame to myself for delaying so long the use of a remedy which my previous experience of a number of similar attacks, added to the nearly uniform testimony of the leading obstetric authorities, had shown to be so unmistakably efficient.

It may be asked whether the hæmorrhage attending the separation and expulsion of the placenta might not have produced the spoliative and derivative effects of bleeding at the arm? To this the reply is obvious, that in the instance under consideration it did not; and, indeed, when we consider that the loss of blood then and there occasioned is almost wholly from the placental cells, and only indirectly maternal, when there is none of the morbid post-partum hæmorrhage of relaxed uterus, no benefit could reasonably have been looked for from this source.

If a marked and permanent improvement in the condition of the sufferer is observed to commence from the moment that the operation of venesection was completed, what other *raison d'être* could be adduced in behalf of the adoption of any therapeutic means, drug, or implement? I may express myself thus forcibly in behalf of a remedy which, in a vast number of the maladies with which we have to cope, is the primary element of rational and successful practice—a remedy which, in these latter days, has fallen too much into desuetude for the benefit of our patients—the primary object of our art. And this remark applies more especially to the rising generation of medical practitioners, who seem afflicted with a wide-spread hæmophobia, leading in acute cases to an expectant or rather aqua-lacteal mode of treatment, the results of which redound very little to their credit, or to that of the medical art as exercised by them.

The removal of the hair is indispensably necessary in every circumstance of severe affection of the cerebrum, not only permitting the effective use of cold applications, but seeming to shut off a demand for capillary circulation in the scalp, which must tend to moderate the head congestion.

The same indication was fulfilled by the purgative powder and enema. The latter, in most cases, can be readily applied, but with the former there is, very generally, much difficulty in its introduction. The subsequent benefit, however, renders it well worth persevering and varied efforts on the part of the medical attendant to secure the object in view.

Dr Simpson observed that the point the author seemed to lay most stress on, was the advisability of bloodletting in eclampsia. He (Dr S.) believed that it was valuable only in such cases where there was evidence of a large amount of blood being in the system, and where labour was near an end. The amount of blood-pressure in the cerebral vessels was thus temporarily reduced, but the blood withdrawn was soon replaced by a more watery fluid, which would bring back the pressure symptoms, unless the whole labour were speedily terminated. He thought Dr Boyd had done what was best for his patient, who was evidently a plethoric young woman, with a full strong pulse.

Dr Pattison remarked, that in his younger days he had seen and bled in many such cases. Now he would depend more on chloroform than any other agent, and would only bleed if the patient was young, strong, and muscular.

Dr Cochrane advocated the advantages of croton-oil, and the repeated administration of equal doses of chloral and bromide of potassium.

Dr Milne thought that no one line of treatment could be laid down for such cases. If the convulsions were due to plethora, bleeding might do good; but if they depended upon uræmia, as was most frequently the case, chloroform and counter-irritation should be employed, while the kidneys might be acted upon by the acetate of potash. As a rule, he found that the convulsions disappeared along with the albumen.

Dr Rattray mentioned the case of a patient who, during the latter part of her pregnancy, suffered much from bronchitis and numbness down her right side. Immediately after her confinement she was seized with most violent convulsions. He administered chloral and acetate of potash in 30-grain doses at short intervals, but without any effect. At last he bled her to 16 ounces, and the fits immediately ceased. He had frequently bled in such cases, and never once had he seen the convulsions return after the bleeding.

Dr Young's experience was not in favour of bloodletting. He trusted almost entirely to chloral, and considered that it was entitled to the first place among the remedies for eclampsia.

Dr Ritchie had never met with a case of uræmic convulsions during labour in his own practice. He had frequently met with cases of albuminuria during pregnancy, some of them being very severe. He attributed his freedom from this complication of labour to the early detection of the kidney disease and the use of remedies, the most important of which he found to be the acetate of potash, combined with iron, purgatives, and rest. He put it to the Society, whether more might not be done by appropriate and early treatment to prevent uræmic convulsions?

Dr Macdonald thought Dr Boyd was quite justified in saying that he had cut short the disease in this case by bloodletting. In many such cases, whether in connexion with utero-gestation or from ordinary Bright's disease, bloodletting was undeniably frequently successful in mitigating and arresting the fits. If we held these fits to be always due to uræmia, it seemed impossible to conceive how the small proportion of the whole amount of poison in the blood removed by the bleeding could act so powerfully. That, however, these convulsions were, in a large proportion of the cases, due to retention in the blood of poisonous materials, had lately received striking support from the exceedingly accurate experiments of Spiegelberg. But that another class of these cases was referable to exalted blood-tension, owing to hydræmia, or hyperæmia possibly acting on an imperfectly-nourished nervous system, was, he thought, equally well substantiated; and it was in this class of cases that bloodletting proved beneficial. But of course there was still the fact to be borne in mind, that those convulsions sometimes stopped of themselves, without any treatment, and that, too, whether they came on before, during, or after labour. He mentioned a case of severe albuminuria giving rise to several convulsions on one occasion, fully a fortnight before labour, and while no pains were present. A

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subsequent ones would be equally so. As showing how labours vary even in the same person, he mentioned the case of a woman in whom craniotomy was performed three times, in whose fourth confinement the child was born before the doctor arrived.

Dr Matthews Duncan would not now discuss the subject of Dr Milne's paper, as he had recently made some lengthy remarks in the Society on the same subject, when Dr Macdonald read his paper. Dr Duncan would repeat, however, that highly-contracted pelvis was a rare thing in Edinburgh and its neighbourhood—much rarer, he believed, than in other places which he could name. He had been well acquainted with obstetric events in Edinburgh for nearly thirty years, and he had never seen or heard of a 2-inch pelvis during that time. A 2½-inch pelvis was also excessively rare. This rarity of highly-contracted brim might be owing partly to the occupation of our people, and partly to the rarity of malacosteon, or of severe cases of rickets. Cæsarian section for contracted pelvis was almost never performed here, while in some Continental localities it was comparatively frequently done. But here we have many cases of slight contraction. Dr Matthews Duncan was struck with the very astonishing success of Dr Milne, both as regards children and mothers, when he induced premature labours, and when the pelvis was believed to have a contracted conjugate—of only 2½ inches in one case; and, to contrast with this success, it had to be remembered that dead-born children in a population were about 1 in 30 or more, and that the mortality of mothers after ordinary labours was very considerable. Delivery of a child which survives, through a 2½-inch pelvis, may no doubt be successfully achieved; but it was also, no doubt, a rare event. He (Dr Duncan) had recently delivered, by premature labour and turning, without great difficulty, a dead child through a pelvis of 3⅜ inches. In former labours similarly treated in skilful hands, the practitioner had, in the same case, used cephalotripsy, and the head had been torn off the trunk. This illustrated what difficulties might unexpectedly occur.

Allusion had been made to Simpson's ingenious arguments and experiments in support of turning in contracted pelvis. But Dr Duncan regarded these as very unsatisfactory, and as now far behind the practical knowledge of the day. Simpson made no careful pelvimetry; he made no distinction of the commoner kinds of contraction of pelvis; he knew nothing of the mechanism of natural delivery in contracted pelvis. There was, therefore, little to be expected from a study of his writings on this subject, now when obstetrics had advanced beyond his position. There could be no doubt that contracted pelves were not of one kind, but of various kinds, which should be diagnosed; and that the treatment should be adapted, not only to the kind of contraction, but also to the other special circumstances of each case. It would never do, at this time of day, to cry out for induction of premature labour and turning, without considering all these matters, and carefully weighing the elaborate works of Spiegelberg, Litzmann, and others.

Authors and practitioners required to be very careful in pelvimetry. Dr Duncan could give many instances of great deception as to the size of a pelvis, even when it was carefully measured; and many great obstetricans had confessed the difficulty of the matter. Of late, however, improvements in this had been introduced; and in every case the accoucheur should do his best.

Part Fourth.

PERISCOPE.

Sulphate of Cinchonia.—The comparative cheapness of sulphate of cinchonia, and its abundance in many of the barks, requires every trustworthy observation on its therapeutical value to be carefully considered. It has formed the subject of a recent communication to the Academy of Medicine by Dr Briquet, a well-known authority on the cinchona alkaloids. This communication deals with the two important questions: (*a*) Has sulphate of cinchonia the same therapeutical action as sulphate of quinia? and (*b*) Is it liable, as has been asserted, to produce toxic effects, when used in the treatment of disease?

In attempting to answer the first question, Dr Briquet adduces the results obtained in 896 cases of intermittent fever treated with sulphate of cinchonia. In 874 of these the disease was cured, and in only 22 was the treatment unsuccessful. Still, there is good evidence to show that, as compared with quinia, this alkaloid is apt to produce disagreeable effects. Contrasting 198 administrations of sulphate of cinchonia with 65 of sulphate of quinia, in which attention was paid to this point, Dr Briquet finds that gastric and cerebral disturbances were more frequent after the use of the former than of the latter alkaloid. He explains this by the fact that the therapeutical action of quinia is more powerful than that of cinchonia, necessitating the administration of such large doses of the latter, that the disagreeable symptoms referred to are produced, notwithstanding the comparative feebleness of cinchonia as a toxic substance. These symptoms may, however, be avoided, 1st, by giving the salt in solution, or if in powder, by causing a glass of acidulated water to be drunk immediately after it has been administered; 2d, by always subdividing the total dose, so that it is taken in small quantities, hourly, during a period of five or six hours; and 3d, by arranging so that an interval of eight or twelve hours is permitted to elapse between the last dose and the expected occurrence of the fit.

As the therapeutical action of cinchonia appears to be less powerful than that of quinia, Dr Briquet recommends its application to be limited to neuralgia, sub-acute rheumatism, and ordinary intermittent fever; while quinia should be used in remittent fevers, and in all cases of malarial disease where life is in jeopardy. Sulphate of cinchonia ought to be given in doses varying from seven to twelve grains.—*Bull. Acad. de Méd.*, Oct. 1872.

CARBAZOTATE OF AMMONIA AS A SUBSTITUTE FOR SULPHATE OF QUINIA.—This salt, otherwise known as picrate of ammonia, occurs when pure in the form of yellowish-red crystals, moderately soluble in water, and less so in alcohol. It has, in various quarters, been proposed as a substitute for quinia in the treatment of malarial diseases, and especially of intermittent fever. Two very interesting papers, containing evidence in support of its efficacy in these diseases, have recently been published by Dr Dujardin-Beaumentz. In the first of these, the author describes six cases of quotidian ague, which were successfully treated with this remedy. He also mentions, as the result of an experimental investigation, that, like quinia, carbazotate of ammonia diminishes the number of the cardiac contractions, and the amplitude of the pulse; produces vertigo, cephalgia, and even delirium, when the dose is a large one (nearly two grains); and also produces cerebral phenomena, which may be designated *picric intoxication*, and which closely resemble those in quinic intoxication. He concludes, (1.) that carbazotate of ammonia acts with great efficacy in intermittent fever; (2.) that the access of the fever may be prevented by the administration of from 0·3 to 0·6 grain in the twenty-four hours; (3.) and that in these doses, it has never caused dangerous symptoms, and seems better supported than sulphate of quinia. In his second paper, Dr Dujardin-Beaumentz describes other seven cases of intermittent fever in which this remedy was used. Three of these were cured, the first being a case of tertian fever, in which 0·75 grain was given daily for six days; the second, one of the same type, in which, during twenty days, the patient received first 0·3 grain, and afterwards 0·75 grain; and the third, one of quotidian fever, in which 0·45 grain was given daily for ten days. In the remaining four, this remedy proved either unsuccessful or only partially successful, and sulphate of quinia had to be administered before a complete cure was effected. They were all of the tertian type. Dr Dujardin-Beaumentz believes that these unsuccessful cases merely show that the remedy is a less potent one than sulphate of quinia, and ought not to lead to its rejection. In those cases where it is efficacious, it will generally cure the disease in doses of from 0·75 to 1 grain in twenty-four hours; but the dose may without inconvenience be increased to 1·5 grain daily. These results are worthy of serious consideration, especially when it is considered that these small doses of carbazotate of ammonia cost but little, while the large doses of sulphate of quinia that are required in intermittent fever are expensive. The bitter taste of carbazotate of ammonia renders objectionable its administration in the form of solution, but it may be conveniently administered in pills, each of which contains 0·15 grain. Five or six such pills may be taken in twenty-four hours without causing any gastric disturbance or other disagreeable effect.—*Bulletin Général de Thérapeutique*, 15th Nov. 1872, p. 385.

Dr Clementi (whose joint researches with Dr Thin, in connexion with the same subject, are recorded in the July Number of this Journal) has made further experiments, with the view of throwing light upon the alleged presence of bacteriæ in the blood of animals suffering from experimental septicæmia. In the previous series of investigations microscopic examination failed to show any such organisms. In order to satisfy himself whether, since bacteriæ were not found, germs capable of generating them might not be present, Dr Clementi experimented with Pasteur's fluid. Following the method of Klebs, he took blood direct from the jugularis, by inserting into it a drawn-out glass tube, which he then closed by melting it not far from the point. Then, after strongly heating a test-tube, he poured boiling Pasteur's fluid into it while the glass was still hot. Breaking off now the point of the glass tube, which was still in the vein, he introduced the closed tube filled with blood an inch into the test-tube, in such a way that the latter was completely closed by wadding with which the wide end of the drawn-out tube was covered. He further sealed the test-tube, so that it was made perfectly impermeable to air. When the Pasteur's fluid was cold, the dilated part of the drawn-out tube was burst by heat applied to the corresponding extremity of the test-tube, and the contained blood was thus allowed to mix with the Pasteur's fluid without the admission of air to the latter.

By this process, experiments were made with blood taken from animals in different stages of the disease produced by putrid and transmitted blood. The test-tubes were opened after five or eight days, and the Pasteur's fluid in no case showed the slightest opacity. Several drops were examined by the microscope, and the blood-corpuscles found very little altered. Minute granular particles were found in small number without independent movement, round or irregularly shaped, and isolated. No bacteriæ or any other organism was found. The same experiment made with the blood of healthy animals also showed the minute granular particles.

If either bacteriæ or their germs had been present in the blood, their further increase or development must have taken place in the Pasteur's fluid; for when the smallest possible quantity of putrid blood was mixed with it in the same way in another test-tube, it was clouded after two days, and on the fifth day was quite milky in appearance. In a drop examined microscopically bacteriæ were found in all stages of development.—*Centralblatt f. d. Med. Wissensch.*, 1873, No. 45.

RESEARCHES AS TO THE PATHOLOGY OF PERFORATING DISEASE OF THE FOOT. By S. DUPLAY and J. P. MORAT.—The following is a brief abstract of researches on this interesting but little-known disease:—

1. *Its Clinical History.*—The following are symptoms—some constant, others very common:—(*a.*) *Lesions of sensibility* are found *invariably* when looked for. In every case there is, in at least one part of the ulcer, a marked absence of sensation; either numbness or absolute anæsthesia. This extends over the part supplied by one branch of nerve extending as far as the nails of the toes, and upwards to a considerable distance in the back of the leg. (*b.*) *The skin and cellular tissue* are altered, cuticle of the sole thickened, scaly eruption on dorsum of foot, marked pigmentation, perspiration increased or altered, or suppressed; subcutaneous cellular tissue thickened and hardened. (*c.*) The *joints* are more or less completely anchylosed, sometimes partially dislocated. (*d.*) There are *skin eruptions* of various kinds; erythema, eczema, abscesses, or even sloughings. Perforating ulcer of the foot is thus accompanied by a certain number of characteristic symptoms which point at some special cause.

2. *Anatomical lesions* on post-mortem examination.—(*a.*) *Skin* is perforated and hypertrophied in vicinity, cellular elements enlarged, papillæ hypertrophied, sweat-glands disappear. Tendons and their sheaths are ulcerated and destroyed. (*b.*) *Bones and joints.* The various stages of ostitis are seen in the bones—rarification, eburnation, separation of periosteum, necrosis. The cartilages of joints inflamed, and eventually are destroyed; a fibrous anchylosis frequently results. (*c.*) *Muscles* show inflammatory changes. (*d.*) *Vascular lesions.* These already described by MM. Péan, Dolbeau, Delsol, Montaignac, etc., have been looked on by some as the initial and most important facts of the disease. From six cases, MM. Duplay and Morat show that the vascular lesion is always an arteritis propagated from the ulcer to the vessels by continuity of tissue; attacking the lining membrane or inner coat of the vessel; extending up it for a considerable distance, and eventually spreading to the middle coat. Atheroma has never been noticed. This lesion of the vessels, far from being a primary lesion, is merely a secondary one of purely local origin, extending from the ulcer. (*e.*) *Nerve lesions*, indicated by MM. Poncet (1864 and 1872) and Estlander (1871), suspected by M. Lucain (1868). These are the most important, and our authors have studied them in six autopsies. To the naked eye the nerve has a gray colour, and in the neighbourhood of the ulcer its external cellular sheath has undergone thickening, and presents swellings and curves. As to the ultimate structure, the tubules undergo a degeneration resembling that which occurs after section of a nerve and its separation from its centre; the connective tissue is thickened, and the vessels assume the embryonic type. A glance at these various lesions shows (1.) That a

degeneration of one or more nerve bundles exists, which affects a greater or less extent of its distribution; (2.) That in the region of its distribution there is an ulceration of varying depth producing destruction of tissues; (3.) That, beyond this ulceration, there exist lesions of an inflammatory character.

3. *Pathology of the Disease.*—Our authors examine the chief theories already advanced as to the nature of the disease, and combat them in turn; Gosselin thought it was an ulceration of skin and synovial membrane—Maurel, an ostitis, or tumour of bone; others thought it epithelioma or cancer; another theory would derive its origin from an atheroma of the arteries, while atheroma is an exception, and the inflammation of the inner coat is a result of the ulcer. What, then, is the real relation between the ulceration and the nerve lesion? Clinical observation and physiological reasoning both agree in showing that the nerve lesion is the *cause* of the ulceration. How does it occur? Is it by the anæsthesia, or is it a lesion of nutrition? This our authors do not determine. What are the causes of the nerve lesion in its turn? These are numerous. Perforating disease of the foot has been found to follow a section of the sciatic nerve; a compression of that nerve or its roots; a disease of the spinal cord; a progressive disease of the muscular system, due to a lesion of innervation, to bruises and crushing of the limbs; in one case to the effect of cold. In a word, if the cause of the nerve injury is not always apparent, the pathology of the perforating ulcer, which is its result, is quite established. The chain of facts is always the same, and it is through nerve lesions that diseases the most diverse, and causes the most distant, produce perforating disease of the foot. On this account, the prognosis must be subordinated to that of the cause; and the treatment in most cases can only be of the most limited and palliative character, directed simply against the merely secondary fact of the ulceration. The following are the conclusions of this interesting work:—(1.) "*Perforating disease*" is an ulcerative affection of the foot, the result of a degeneration of the nerves of the part. (2.) The degeneration of the nerves which causes the ulceration may itself be the result of very varied causes; such as the injuries of the spinal cord or ganglions; section or compression of the great nerve trunks; alterations in the ends of the nerves. (3.) The ulcer, once begun, is accompanied by an inflammation which affects all the tissues of the region, and may extend its effects to a considerable distance.—From an abstract in No. 22 of *Lyon Médicale*, from the original papers in the *Archives Générales de Médecine*, March, April, and May 1873.

EPIDEMIC OF GOITRE IN A BARRACK.—More than 200 cases of goître have appeared among the young soldiers at the barrack of St Etienne. The following is the essence of the report on the epidemic by M. Bergeret:—This epidemic cannot, like other ones, be attributed to an excess of sulphates in the drinking water, for it is as pure as can possibly be imagined—it is too pure. It gives no precipitate in the salts of baryta, or with those of silver, or with ammonia; the photographers use it as if it were distilled water. It is rain-water, which rushes down the primitive rocks of Mont Pilatus. The cause of the goître seems to be an excess of sulphates sent into the circulating blood, as a result of an exaggerated muscular atrophy, which, in turn, is the result of too much forced exercise. The receipts of the system do not equal the waste. Besides, it is well known that when a muscle works forcibly and continuously, or when it is submitted for a time to a continued electric current, the muscle, burning up its own substance, becomes acid—the acids produced being the sulphuric and phosphoric acids—at the expense of the sulphur and phosphorus of the albuminous elements. In the condition of excessive labour, a man has in his circulation an abnormal quantity of sulphates, just as if he was drinking impure waters. This is what has taken place in the case of the soldiers with goître, and it is confirmed by an analysis of their urine.—*Gazette Hebdomadaire*, No. 41, 1873.

AMPUTATION IN AGED SUBJECTS.—Dr H. A. Spencer, of Erie, Pa., gives, in the *Reporter* of 11th Oct. 1873, the report of a case of amputation, with recovery, in a patient 75 years of age. He reports the case on account of the age of the patient. A case of amputation of the leg, with recovery, occurred in my practice, in Erie Co., N. Y., in a patient aged 78 years and some months. On 30th July 1867, I was called to see Mr S. R. Russell, who had accidentally stood in the way of the knife of a mowing-machine in motion, and one of his legs was cut off at about the junction of the lower and middle third. His son, who was riding the machine, had the presence of mind to grasp the stump, and, by compressing it with both hands, prevented bleeding. He held on while the patient was moved from the field to the house, and until my arrival, when I relieved him by application of the tourniquet, and proceeded at once to amputate. The patient was nearly killed by the chloroform, which was administered by an old physician who happened to be passing, and was called in at the opportune moment. Stimulants, friction, and artificial respiration revived him. Gangrene occurred in the stump after a few days, but yielded to bromide, Laborraque's solution, and yeast poultices. About three months afterwards, while trying to walk with crutches, he fell, striking on the hip opposite his injured side, and sustained an intracapsular fracture of the femur. This did not kill him, and he lived until the fall of 1872, when he died of epidemic acute dysentery. R. J. CURTIS, M.D.—*Philadelphia Medical and Surgical Reporter.*

EXTIRPATION OF KIDNEY.—Dr Simon, of Heidelberg, reports a case of the removal of the kidney on account of calculi. A female, aged thirty, had suffered for eighteen years from pain in the left kidney, which extended down to the bladder, and was aggravated by movement. Six years after their first appearance the pains became regular in their occurrence, appearing about every six weeks, and lasting for five or six hours. As time went on they became more frequent, and lasted longer, causing early abortions on two occasions.

On examination of the abdomen, the left kidney was tender on pressure. Nothing was made out by pulsation or percussion. The urine was turbid and contained pus. After each attack it contained blood, epithelium, and small calculi of phosphate of lime. Simon determined to operate, seeing that drugs had failed, and the patient was excessively anxious for relief. The pain had always been on left side. He made an incision in the loin, and arrived at the fat round the kidney. On touching the kidney with the finger, it felt uneven, but no calculus could be felt from the outside, and the ureter was not dilated. The simple incision had then to be given up, and extirpation attempted. The kidney was then picked out of its capsule, and the pedicle tied with a double thread. The substance of the kidney did not contain any calculi, but there were eighteen or twenty in the pelvis. The wound was filled up with charpie. For twenty-one days the patient did well, appetite improved, the urine was normal in quantity, the general health was good. On the twenty-first day, after an examination of the wound with the finger, the temperature rose suddenly, shiverings followed, and she died on the thirty-first day, with symptoms of peritonitis and double pleurisy. At the autopsy the wound was found practically healed, well filled up with connective tissue. The right kidney was hypertrophied, though normal.—*Dr Nepveu, in Gazette Médicale de Paris*, No. 45, 1873.

TWIN-PREGNANCY AFTER OVARIOTOMY.—Professor Francesco Marzolo of Padua performed ovariotomy (for the sixteenth time in Italy) in July 1871, on a woman aged thirty-four. He removed the left ovary. The patient got well, recovered strength, and returned to work; about a year afterwards, symptoms of pregnancy appeared, and gestation went on without any bad symptom. After a labour of two hours she was delivered of a girl (head presentation). In a short time a boy was born (a footling); both infants survived. This is, we believe, the first example of a twin-pregnancy after an ovary has been excised, and opens up some interesting physiological questions for discussion.—*Gazette Médicale de Paris*, No. 44, 1873.

A CASE OF CORNU HUMANUM OF EXTERNAL EAR.—The disease occurred in a man aged 55. It was of two years' growth. The patient was in the habit of picking it till it bled in its earlier stages, and afterwards of reducing its bulk by paring with a razor. In January 1871 it presented the following appearances:—"A blunted horn-like protuberance three-fourths of an inch long, and nearly as broad at its base, sprang from the upper and posterior portion of left helix. Of a whitish colour at its base, it gradually grew quite smoky at its summit, which was more or less jagged in appearance. It was distinctly striated, the markings running in a slightly divergent direction from the summit to the base. At the extremity and in the middle portion it was hard like horn, but near the base it could be easily compressed, though yet comparatively hard. The line of demarcation between the growth and the normal skin was very abrupt. It was not a source of pain to him, nor was there any tenderness on pressure." It was removed then, and on 1st July there were no signs of its return. "A small sharp point, however, similar, he says, to the one first noticed in the ear, can be distinctly felt on the outer surface of the left eyelid."—Albert H. Buck, M.D., in the *Transactions of the American Otological Society*.

A very similar case came under our notice the other day in the Ear Dispensary here. The structures divided on removing it were very brittle, breaking up rather than cutting before the knife; considerable bleeding took place.—J. J. K. D.

STATISTICS OF GUNSHOT WOUNDS OF THE HIP-JOINT.—In the second congress of the German Society of Surgery, Langenbeck opened a discussion on the above most important subject, and gave his experiences. He had seen several cases of recovery from this injury, which by some authors is regarded as a mortal one, and he believed that in the first forty-eight hours it should be determined whether operation is necessary or not. Diagnosis is generally easy of injury of the joint, from the rapidity with which infiltration takes place, the swelling of the joint which makes the femoral artery project in front, the escape of pus and synovia by the wound when the joint is pressed on, and the position of the limb, which is flexed and rotated outwards. Langenbeck has known ten cases recover, seven of these in his own practice. Ice applied to the joint, plaster-of-Paris bandages, and extension by a weight to the foot, are the methods he adopts. Resection of the joint has in the last war given as bad results as it did in the American war. For extensive lacerations of the soft parts, with fracture of the upper third of the femur, primary amputation at the hip-joint should be performed, as none of those treated by the expectant method have recovered.

After Langenbeck, other members of the Society expressed in turn their opinion on this impertant subject. On the whole, these facts hardly warranted so favourable conclusions as did those of M. Langenbeck. The reporter has tried to sum up the cases in a statistical form. Of 65 gunshot wounds of the hip-joint, treated on the expectant plan, 52 died, 13 recovered. In the 13 are the 10 cases seen by Langenbeck. Of 39 treated by resection, 32 died, 7 recovered. Of 13 of these done as secondary resections, 4 got well. Of 15 amputations at the hip-joint, all died. In a word, of 119 cases of gunshot wound of the hip-joint treated in various ways, only 20 cases got well, 99 having died.—*Dr Nepveu, in Gazette Médicale de Paris*, No. 44, 1873.

Dr Joy Jeffries of Boston reports the following interesting cases in the Transactions of the American Ophthalmological Society for 1873:—1. Two cases of intraocular tumour treated by enucleation, one in a female of forty, the other in a man aged sixty. 2. Two well-recorded cases of herpes zoster ophthalmicus, which had destroyed the eyesight. A short account of what is known of this rare and dangerous disease is given, including a résumé of Dr Albert Hybord's monograph on the subject. 3. A rather obscure case of traumatic rupture of the choroid, without direct injury of the eye.

Grafting the Skin of a White Man upon a Negro. By G. Troup Maxwell, M.D., New Castle, Delaware.—I was called in February 1872 to see James Pearce, a negro, who had been shot in the face by the accidental discharge of a gun. The gun was loaded with bird-shot, and, as Pearce was only a few feet from its muzzle, the charge passed through the left cheek almost in as compact a mass as a solid ball, entering just at the outer border of the orbicularis muscle. The shot made a wide and deep furrow through the soft parts, completely severed the masseter muscle in its middle, and escaped at the posterior border of the ramus of the inferior maxillary bone, just below the lobule of the ear. The buccal mucous membrane and the bone were uninjured; but the wound involved all the structures lying exteriorly to them, and as, by retraction of the severed tissues, the lateral dimension of the wound was considerably increased, it presented a horrible appearance, the ghastliness of which was increased by the charred and blackened effect of the burning powder.

An effort was made to contract the limits of the wound as much as possible, by approximating its sides as closely as could be done with adhesive strips and bandages; but, notwithstanding all the care that was taken, the sloughing was so extensive that there resulted a deep, disfiguring wound, of irregular shape, and covering nearly the whole left cheek.

To expedite the healing process, and to prevent, as far as possible, the disfigurement of an extensive cicatrix, I proposed to graft skin into the wound as soon as it presented a healthy granulating surface. This proposition having been assented to, I performed that operation in March, about four weeks after the injury was received.

Whilst making preparations for the operation, the idea of grafting the skin of a white man upon a negro occurred to me; and, having obtained the consent of my patient to the procedure, I clipped from my own left forearm a piece of skin about the size of a dime, for the purpose. I also took a piece from his forearm, intending to graft both white and black skin.

These pieces of skin were divided into small fragments about the size of canary-seeds, and four of each kind were carefully inserted into the surface of the wound.

On the tenth day, the bandages and strips of plaster which held the grafts *in situ* were removed, and six of the eight grafts were discovered to possess vitality and to encourage the hope of success.

Unfortunately, that day, after the dressings had been changed, my patient got on a "spree," and, whilst intoxicated, destroyed four of the grafts. Happily for the success of my experiment, of the remaining two, one was white. These grafts grew finely, extending themselves in all directions, and assisted materially in closing the wound, which was quickly done.

Besides the wish to heal the wound as speedily as possible and with the slightest amount of cicatricial contraction, considerable interest was felt in the result of the experiment—which, so far as I am informed, had never been done before—of grafting the skin of a white person upon a negro. It was a source of gratification, therefore, to witness the growth of the white graft, and I watched its progress with peculiar pleasure. From the size of a canary-seed, it increased to the extent of about a half-inch in its greatest dimensions, and was of irregular form, with narrow points extending into the surrounding black surface.

Meeting Pearce on the road after the wound had healed, I readily distinguished the white patch in the side of his face, twenty to thirty yards distant. Upon examination at that time, dark-coloured lines, forming a network in the white skin and giving it a purplish tinge, were discerned. These, I supposed, were bloodvessels. These vessels increased in size and number, producing a corresponding deepening of the colour of the patch, until by the end of the third month the whole surface of the wound was of a uniform blackness. The white skin had lost its distinguishing characteristics.

Does not this experiment prove that the colouring-matter of the skin, its pigment, which varies in quantity in individuals as well as in races, is due to cells which are developed in the liquor sanguinis, and not to any peculiarity in the cells of the skin itself?—*Philadelphia Medical Times.*

ASPIRATION IN RETENTION.—The following are the notes of two cases in which aspiration was employed to relieve over-distention of the bladder in retention.

Case I.—G. B., æt. 13, three days before admission, slipped from an iron railing on which he was standing, striking on the perineum. He experienced acute pain in this locality, and shortly afterwards, on attempting to urinate, found himself unable to do so.

His family physician failed on attempting to introduce a catheter. On admission, patient says that he has not passed water in two days; is suffering very acutely from abdominal pain and desire to micturate; bladder enormously distended, extending above the umbilicus. Perineum red, swollen, and painful. Catheter meets obstruction five inches from the meatus. Dieulafoy's pneumatic aspirator being obtained, a small-sized trocar is introduced in the median line of the abdomen, an inch above the pubes, and a large quantity of urine withdrawn, some being left in the bladder, for fear of hæmorrhagic cystitis, which is apt to arise if the bladder be too suddenly relieved of its contents. External perineal urethrotomy was performed, and an extensive rupture of the urethra found at the membranous portion. An incision was made in the wall of the canal anteriorly and posteriorly to the point of rupture. Two months after the operation, the perineal wound had closed, and a No. 10 sound passed without difficulty into the bladder.

Case II.—J. W., æt. 18. Patient states that he had gonorrhœa two years ago. Within last two weeks has had considerable difficulty in passing his water. On the morning of the day of admission he was seized with retention, and when admitted, late in the evening, had not urinated in twenty-four hours. On admission, patient is suffering acutely from constant desire to urinate. A catheter is introduced, passing a slight stricture at the meatus, and meeting obstruction three inches from the meatus at a second stricture. Efforts to pass through this failed, and, the patient suffering greatly, the aspirator was employed to relieve him.

Steurer's modification of Dieulafoy's instrument was used, and a large quantity of urine withdrawn, the point of puncture being as in Case I., the patient experiencing immediate relief, unmindful of the slight pain caused by the puncture. A whalebone guide was passed next day, and forcible dilatation made.—*Philadelphia Medical Times.*

In favour of a New Method of Healing Ulcers.—Dr Nussbaum, in the *Wien. Med. Press*, claims to have successfully treated upwards of sixty cases of chronic, extensive, and otherwise intractable cases of leg-ulcers by the following simple procedure. The patient is at first narcotized, and then around the ulcer of the leg or foot, a finger's breadth from its margin, an incision extending down to the fasciæ is made. Numerous bloodvessels are divided, and a severe hæmorrhage ensues, unless a fine pledget of lint be packed into the cut and the entire ulcer strongly compressed. The packing with lint is also necessary to prevent union of the cut edges by the following day. Upon the second day the bandage and lint are removed. From then until a cure is effected a simple water-dressing is applied. The author states that an astonishing change can be seen even in the first twenty-four hours. The ulcer which yesterday threw off quarts of thin, offensive, ichorous pus, furnishes to-day not more than a tablespoonful of thick, non-offensive, healthy pus. The old ulcer becomes rapidly smaller, healing from the margin towards the centre, and is healed in a short time; but the cut is changed into a broad circular sore, which also speedily cicatrizes. The great diminution of the secretion and other favourable changes occurring in the ulcer find an explanation from the fact that the circumcision has divided dozens of large, abnormally-widened bloodvessels. Time is thus given for the lessened nutritive material, which previously was carried off by the excessive secretion, to be transformed into cells and connective tissue; in other words, granulations are formed, which fill up and heal the deep ulcer. Without claiming this as a radical method, the author assures us that the cure is much more rapid, and the cicatrix becomes more elastic and resisting, than in the ordinary means applied, which usually require so much time that the patients depart with half-cured ulcers, soon to find themselves in their previous deplorable condition.—*Philadelphia Medical Times.*

Part Fifth.

MEDICAL NEWS.

DEATHS FROM ETHER.—Dr Kidd writes, "I see the *Boston Medical Journal* is very severe, but insufficiently informed, as to my statement (page 347) that forty-one deaths were 'enquired into' by its committee, and that I wished to convey thus the idea that all these forty-one were from ether *alone.* I explained, in fact (as it now does), many years ago in print, on studying the 'report' carefully, that not more than ten or twelve were from ether alone, and the rest possibly from ether and effects of tedious exhaustion and slow hæmorrhage induced by the ether: but some ten or twelve were on the operation table, or immediately on removal from it. All I asked of the *London Medical Record*, which took up the strong position that there never was a single death from ether, was, that in fairness it should refer to this forty-one list, and seven similar deaths after ether at Lyons, as to which, by some typographical or incredible mistake, it stated exactly the opposite of what was the fact, treating this list of forty-one as altogether non-existent also. It is quite clear there have been now over fifty deaths directly due to ether or to this tendency to deep exhaustion, secondary hæmorrhage, or collapse which it induces. A sophistical method is now adopted, as the Boston list can no more be denied, to visit the false suggestion and misstatement, not on the *Association Journal* and *Record* inspired by the same pen, but on me, pretending that I ascribed all these forty-one cases directly to ether; whereas I was careful to note that there were only ten or twelve. I studied the list, in fact, with great care, and followed up these seven cases at Lyons."—*Students' Journal.*

TO KEEP GUM ARABIC FROM MOULDING.—Solutions of gum arabic soon mould and sour, and finally lose their adhesive property. It is said that sulphate of quinine will prevent this, while it imparts no bad odour of its own. The addition of a few crystals of this salt to gum arabic will prevent the formation of mould quite as effectually as carbolic acid; and by analogy it is safe to suppose that the same salt could be used in writing-ink, mucilage, and possibly glue.—*Druggists' Circular.*

ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH.—At the annual election of the Royal College of Physicians of Edinburgh, on Thursday the 4th December 1873, the following office-bearers were elected for the ensuing year:—*President*—Dr William Henry Lowe. *Vice-President*—Dr R. Paterson. *Council*—Dr Andrew Halliday Douglas, Dr Robert Paterson, Dr Alexander Keiller, Dr James Matthews Duncan, Dr William R. Sanders, Dr George William Balfour. *Examiners*—Dr Bell, Dr Douglas, Dr Robert Paterson, Dr Wright, Dr Keiller, Dr Pattison, Dr Cumming, Dr M. Duncan, Dr Haldane, Dr G. W. Balfour, Dr Struthers, Dr Ritchie, Dr Stewart, Dr Muirhead, Dr Angus Macdonald, Dr T. R. Fraser, Dr M'Kendrick, Dr Pettigrew. *Treasurer*—Dr Samuel Somerville. *Secretary*—Dr Daniel Rutherford Haldane. *Curator of Museum*—Dr Thomas R. Fraser. *Librarian*—Dr George William Balfour. *Registrar of Applicants for License*—Dr Daniel Rutherford Haldane. *Clerk*—Mr Christopher Douglas, W.S. *Auditor*—Mr Kenneth Mackenzie, C.A. *Under-Librarian*—Mr John Small. *Officer*—Thomas Marshall.

THE following gentlemen were elected as office-bearers of the Medico-Chirurgical Society of Edinburgh for the ensuing year at the meeting of 3d December 1873:—*President*—Dr D. R. Haldane. *Vice-Presidents*—Dr R. Paterson, Dr P. H. Watson, Dr G. W. Balfour. *Councillors*—Dr Sinclair, J. D. Pridie, Esq., Dr M'Kendrick, Dr Cadell, Dr Niven, Dr Cuthbert, John M'Gibbon, Esq., Dr Andrew. *Treasurer*—Dr Joseph Bell. *Secretaries*—Dr Muirhead, 7 Heriot Row; Dr Chiene, 55 Castle Street.

EDINBURGH OBSTETRICAL SOCIETY.—The following gentlemen have been elected office-bearers for the sessions 1873-75:—*President*—Dr J. M. Duncan. *Vice-Presidents*—Dr Angus Macdonald and Dr R. Peel Ritchie. *Treasurer*—Dr James Young. *Secretaries*—Dr Andrew and Dr Milne. *Councillors*—Dr L. Thomson, Professor Simpson, and Dr Wilson. *Librarian*—James Jamieson, Esq., L.R.C.P. and S.

ROYAL MEDICAL SOCIETY.—At a meeting of the Society on the 28th November, the following gentlemen were elected Presidents:—G. T. Beatson, B.A.; Alexander James, M.B., C.M.; J. Stevenson Forrester, L.R.C.P. and S. Ed.; C. Watson M'Gillivray, M.B., C.M., L.R.C.P. and S. Ed.

BOTANICAL SOCIETY OF EDINBURGH.—Office-bearers, December 11, 1873:—*President*—Sir Robert Christison, Bart., M.D., D.C.L., Professor of Materia Medica. *Vice-Presidents*—William Craig, M.D., C.M.; Jas. M'Nab, Esq.; Sir Walter Elliot, K.S.I., F.L.S.; Professor Dickson, M.D., F.R.S.E. *Councillors*—John Kirk Duncanson, M.D., C.M.; Chas. Jenner, F.R.S.E.; A. P. Aitken, M.A., D.Sc.; Patrick D. Swan, F.R.S.E.; Hugh Cleghorn, M.D., F.R.S.E.; Alexander Hunter, M.D.; David Grieve, F.R.S.E.; William Gorrie, F.R. Cal. Hort. Soc.; Isaac Anderson-Henry, F.L.S., F.R.S.E.; Thomas A. G. Balfour, M.D., F.R.S.E., F.R.C.P.E. *Honorary Secretary*—Professor Balfour, M.D., F.R SS. L. & E. *Honorary Curator*—The Professor of Botany. *Foreign Secretary*—Professor Maclagan, M.D., F.R.S.E. *Treasurer*—Patrick Neill Fraser, F.R. Cal. Hort. Soc. *Assistant-Secretary*—John Sadler, F.R.P.S.

The following tribute from a great American surgeon to the late Professor Miller will be read with interest:—

MILLER MEMORIAL HOME.—(*To the Editor of the Philadelphia Medical Times.*)—Dear Sir,—Nearly two months ago I received from Dr A. G. Miller, son of the late Professor Miller, of Edinburgh, a copy of the subjoined circular,[1] inviting the co-operation of the medical profession of the United States in furtherance of the object therein stated.

In consequence of absence from the city and of various pressing engagements, I was reluctantly compelled to remain silent until the present moment. I now call the attention of my professional brethren to the subject, and sincerely hope that the appeal will meet with a prompt and hearty response.

There is the strongest reason why such an appeal should be generously met on this side of the Atlantic. For nearly a quarter of a century the surgical writings of Dr James Miller, for many years Professor of Surgery in the University of Edinburgh, were the text-books of the medical students of our schools, and the works that were daily consulted by our practitioners, not only in cases of emergency, but as a part of their daily reading and contemplation. Both his "Principles of Surgery" and his "Practice of Surgery," edited by Dr Sargent, of this city, passed, if I mistake not, through four editions in this country, and enjoyed for a long time an unbounded popularity. Even at the present moment they are used as text-books in several of the American schools of medicine. It is not too much, then, to say that the family and friends of Professor Miller have a substantial claim upon our gratitude. But this, although a very important one, is not the only reason why we should extend a generous hand to this praiseworthy enterprise. Every contribution that is made in aid of such an object in behalf of a British professional brother at once assumes the character of an international courtesy, and, as such, it cannot fail to strengthen the bonds of good-fellowship between the two nations.

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[1] This circular appeared in the Journal for September, p. 282.

The late Professor Miller was not only a great writer and teacher, but a most accomplished practitioner, a Christian gentleman, and a warm-hearted philanthropist, never wearying in his efforts to suppress vice and immorality, and to improve the condition of the humbler classes of people in his own and in foreign countries.

Subscriptions may be remitted directly to Edinburgh, to the address of Dr A. G. Miller, or to Dr Barton, to my care, Eleventh and Walnut Streets. Medical journals friendly to the object, will please copy.

I have the honour to be, very respectfully, your friend and obedient servant, S. D. GROSS.

PHILADELPHIA, *October* 15, 1873.

REALLY TOO KIND.—More than one of our contemporary medical journals have not only had the kindness to sympathize with the great loss which the University of Edinburgh has sustained in losing Professor Bennett's valuable services as a Professor, but have even shown additional courtesy, in the selection of his successor in the Chair. We are glad to inform them that Professor Bennett has not resigned his Chair, and we trust that it may be long before he does so; also, that there is no lack of suitable candidates from which to choose his successor, should one ever be required; and also, that the Edinburgh Medical School, notwithstanding the losses it has sustained, was never in such a healthy and flourishing state as at present.

OBITUARY.—By the death of Professor Agassiz, which occurred on the 14th December at New York, science generally, and natural history in particular, has lost one of its most industrious workers and successful exponents. He was educated in medicine in Switzerland, but soon devoted himself to natural science, becoming Professor of Natural History first at Neufchatel, and afterwards in Boston. Glaciers; fishes, recent and fossil; echinodermata, and mollusca, formed the subjects of some of his earlier monographs. More recently, he worked at physical geography and zoological classification, perhaps his most important work being "Contributions to the Natural History of the United States." Genius, industry, and natural love of the subject, combined to make Agassiz one of the most successful cultivators of Natural Science.

SCALE OF FEES adopted by the MEDICAL SOCIETY OF VICTORIA, July 1872:—

GENERAL.

	£	s.	d.
Single visit, or advice in which no further attendance is likely to be required	£1	1	0
Night visit or advice (between 10 P.M. and 7 A.M.) in which no further attendance is required	2	2	0
Ordinary visit or advice	0	10	6
Ordinary night visit or advice	1	1	0
Visit or advice, with special examination	1	1	0
If more than one mile from the practitioner's residence, for each mile in addition	0	10	6
During the night	1	1	0
When the visit is to more than one person in the family, and the fees are all paid by one person, half the sum to be charged for each beyond the first.			
Visit or advice in consultation with another practitioner	1	1	0
Written advice	1	1	0
An opinion involving a question of law	10	10	0
A *post-mortem* examination	4	4	0
Certificate of health	1	1	0
Vaccination	0	10	6
Midwifery and miscarriages, £3, 3s. and upwards.			
Special visit in the day-time	1	1	0
Administering chloroform	2	2	0

SURGICAL.

	Guineas.	
For Reducing fractures	2	to 5
... Reducing dislocations	2	to 20
... Passing the catheter	2	
... Lithotomy or lithotrity	50	to 100
... Major amputations	25	
... Minor amputations	5	
... Extirpation of tumours	5	to 25
... Excision of joints	25	
... Trephining	10	
... Grave operations on the eye	20	
... Ligaturing arteries in aneurism	25	
... Operating in hernia	25	
... Paracentesis thoracis vel abdominis	5	
... Operating in hare-lip or cleft palate	5	to 20
... Operating in fistula in ano	5	to 10
... Removing hæmorrhoids	5	to 10
... Operating in hydrocele	2	
... Tracheotomy	10	to 20
... Minor operations on the uterus	5	to 10
... The Cæsarian section	50	
... Ovariotomy	50	to 100
... Vesico-vaginal fistula	50	

N.B.—These fees not to include after attendance, nor the payment of assistants.

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Henry Blanc, M.D.,—Cholera, how to Avoid and Treat it. London, 1873.

S. M. Bradley, F.R.C.S.,—Manual of Comparative Anatomy and Physiology. London, 1874.

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Dr P. de Pietra Santa,—La Crémation des Morts en Italie. Paris, 1873.

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Andrew Wood, M.D., — Don Carlos, by Schiller. Edinburgh, 1873.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Extension and Increase of the Scientific Spirit in Medicine.* Inaugural Address to the Obstetrical Society of Edinburgh, by the President, Dr J. MATTHEWS DUNCAN, 10th December 1873.

THE medical profession is called a brotherhood, and although the relationship of its members thus implied is not exactly true, yet I know no word which so fitly expresses the bond which there is among us. It is a very strong union, which begins to be formed in the dissecting-room, and grows closer as life advances. Among practitioners in large towns it shows itself in the formation of medical societies; and many essays have been written to demonstrate their utility, but I think it scarcely needs proof. Meantime, I merely remark that such societies are at once a result of this brotherhood, and a useful fosterer of it.

Under various designations, medical societies exist in large numbers all over the country, and they are yearly increasing. More and more members are enrolled; and, as a necessary result, they have more and more power and influence. The great object which they have before them, is to raise medicine to the dignity of a true science. This position it cannot yet be said to hold, except feebly or in part, but there is no doubt it is quickly growing towards this high rank; and no surer sign of this growth can be found than the manifest increase of the use of scientific method. Scarcely any number of a medical journal can be taken up without evidence of this improvement. It is not altogether made up of long cases, new cures, and mere dialectical disquisitions; but there are arrays of statistics, microscopical inquisitions, chemical researches, physical inquiries—all showing the real quarrying intention; not mere scholastic beating of the air, and varnishing of ignorance with glowing words; not mere assertions of cures or statements of great achievements, but much humbler and better work.

It is not to the present position of medicine as a science, nor to the proper method of advancing it, that I wish at present to direct attention, but to a subject very nearly allied, namely, the spirit or tone of medical societies, medical consultations, medical journals, medical biographies. I feel it almost presumptuous in me to discuss this subject before you, and I assure you I do it with no self-righteous complacency; but I happen to be, for the day, ***primus inter pares***, and I make this use of my position, believing that good will result, not so much from active correction or censorious faultfinding, as from simply throwing light on what I regard our chief fault as a medical brotherhood,[1] a world-wide society called the medical profession.

Before commencing, I must guard myself against being misunderstood. Because, although I am to dwell on a fault, I am not insensible to the paramount excellencies of the medical profession. I avow myself a thorough Æsculapian. I have no wish to be anything else, and I need have no such desire, for I belong to a profession which, while it equals any other in the virtues and worth of its members, is second only to one in respect of the grandeur of the objects at which it aims.

I shall, in the course of my remarks, speak of philosophy and science as antithetical, and I believe I shall not be misunderstood; but, seeing that, if we adhere to the strict meanings of these words, the antithesis is unjust and absurd, it is well to explain what is meant. Nothing essentially disrespectful to true philosophy and true philosophers is suggested. They treat of such subjects as theology, moral philosophy, metaphysics, and may show much learning and ingenuity. In a scientific manner, they as yet prove almost nothing. Everything hinges finally on an assumption or an argument based on assumptions. The contrast with science and scientific men is almost as great as one can conceive. These deal with such subjects as physics, chemistry, biology. No doubt, all of these subjects are surrounded by a philosophical cloud of hypotheses and disquisitions more or less luminous, but their basis is in facts or truths, which can be at any time verified by observation, experiment, or by conclusions directly drawn from these.

The true philosopher has his own sphere, one of vast importance, scarcely traversed as yet in any part by scientific method. He reaches conclusions which may be of great value both theoretically and in the conduct of life. But his ways are quite foreign to those of a truly scientific man.

When we look around the world we cannot fail to remark that all the greatest recent achievements, almost all the most valuable progress of the human race, are due not to philosophy or philosophic methods, but to science and scientific methods. The chief increments of our knowledge, and the new appliances of civilization, are the direct products of the scientific mind of recent times; and who can doubt that, at least in the immediate future, this will be fully as true as it has been in the last century?

Theology, politics, the social sciences, and the higher arts, may still be classed as cultivated by philosophic methods; and while

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[1] "Attamen nescio quo fato vulgus medicorum in theorias hypotheticas semper præceps ruit. . . . Puduit medicos umbras opinionisque commenta sectari jamdudùm à physicâ Newtonum Paschaliumque viribus expulsa."—*Laennec.*

they naturally, in consequence of their admitted paramount importance, show great signs of effervescent life and activity, they have very little of substantial value to exhibit as the result of all the bustle. No doubt the philosophers can show fine qualities of head and heart, and spectators may justly admire their ingenuity and their prowess. But it may be sadly asked, *Cui bono?* They have little to show but broken heads as the fruit of it all. They begin to seem out of place in the nineteenth century, and to excite in us such curiosity as would arise were we to behold now a real tournament, with all the old bravery, transacted before us.

Medicine, the healing art, is such a complex and difficult study, and has made so little progress in the way of exact science, that it seems to halt undecided, midway between the field of the philosophers and that of the scientific men. Medicine has among its cultivators numerous men of great ability, who, from various reasons, probably easily to be laid bare, are enticed into mere dialectics and *a priori* discussions—mere repetitions, with slight alterations, of what has been many times said before. The too slow but invaluable advances of science are ever overthrowing fresh-sprouting philosophical theories, which spring chiefly from the old stump, and are made to suit the new discoveries. These grandiose philosophical structures go, in their exuberance, far beyond the limits of useful theory and hypothesis, and have a very injurious influence, meantime, in medicine at least, quite taking the wind out of the sails of the humble, simple, scientific worker.

A great change for the better is rapidly approaching. I ardently long for it, and I shall do what I can to hasten its advent. Already, everywhere, the scientific method is accumulating stores of facts; much labour is spent in classifying them or putting them in more or less convenient order. The true philosophical fruits will soon, in the hands of some genius, the gift of God, be produced by their further cultivation.

It will greatly favour the rapid progress of medicine, if each of us, singly, or as units in medical societies, keeps this in mind, and eschews premature philosophizing; adopts the quiet, sure plans of the scientific man, not the heaven-assaulting methods of old or new philosophers.

The philosophical method is far too ambitious or self-conceited, not in its aims or hopes, but in its fundamental principles or plan of work. It assumes power in the human intellect above what experience has shown it to possess. It seems to assume, as some great philosophers have avowedly done, that there is innate in the human intellect a latent original power, which, exercised under proper circumstances, and with due care, will lead a truly great philosophical mind, without the aid of observation and experiment and induction, to the high thrones of general truths or laws which may be used to verify the results of slower scientific methods, or even to obtain them. "The mighty intellect of the philosopher," says an eminent physicist, "disdains all peddling with apparatus. To it all truth is intuitive, and what it cannot comprehend cannot be truth." No doubt there is a boundless field for the development of metaphysics and mental philosophy and logic; both as advancing these important departments of human knowledge, and as affording, what has hitherto been the chief occupation of their cultivators, ample room for a high and mighty exhibition of intellectual gymnastics, by intellectual acrobats; and, if we judge by results, little else is to be expected.

It is for the scientific man to take a truer view of his powers, at least until the higher has been proved good in theory and in practice; to aspire to knowledge of the works and ways of the great Creator by the slow ascertainment of fact and of law; to eschew the assumption of innate undemonstrated powers; to believe, not in the perfection of the human mind, but in its capability of growth and evolution; to believe that honest labour at details is the basis of progress in science, and that the intellect is not stunted and atrophied by waiting for opportunities, for the call of a genius, or the touch of a magician; but that, like other things in nature, it is subject to a law of progress, probably like that of organic beings, as has been so grandly conceived by the author of the "Origin of Species;" that the intellect is not, and never can be, perfect, but may go on and on towards perfection. We must work from below upwards, and avoid the pompous and seductive and popular pride-satisfying way of the philosophers.

Darwin, a scientific man, propounds a hypothesis which, however far from being proved, or from being sufficient, is a generalization from a vast array of facts, is of great utility in its own place in science, and has led indirectly to many important inquiries, and to much scientific progress. He is assailed by scientific men who disagree with his hypothesis, and rest their arguments on a more or less scientific basis. But he is not merely assailed, he is also vilified and reprobated by the philosophers, who, without any basis, except of thin fancies, attack him with dialectical weapons; and one of them, much distinguished in his class, advances, in reply to Darwin, his theory of the world. It is, says he, but the representation in externality of reason that is the internality, and is reducible or construable into it. I ask you only to look at the two pictures, and say whether or not you prefer science in its winged flight, or philosophy in its vesture of learned profundity.

Almost everything great in medicine has been done by the scientific method, while the works of medical philosophy lie thickly around us, chiefly as mere encumbrances. This is particularly well observed in the department of therapeutics. We cannot expect to see ourselves as others see us, but we may be assisted in seeing ourselves aright by looking at some departments of ancient therapeutics where our view is unclouded by prejudice. A recent reviewer has used this plan to good effect, showing that many, if not most, of the drugs and cures of great men of ancient times which have so grotesque or so revolting an appearance to us, and which are no doubt nearly or altogether useless, have their exact scientific, or rather non-scientific, counterparts in many if not most of the drugs and cures of great men of modern times; both having no good evidence in their favour, but having their small utility philosophically founded on mere assumptions, credulity, authority, or so-called experience.

The cultivators of medicine, its best and most scientific practitioners, will, unless very favourably situated, or possessing extraordinary passports to success, be always beaten in the race of life, in getting on, as this is vulgarly estimated. The *profanum vulgus* cannot appreciate the simplicity and accuracy and truthfulness of the rare scientific practical man. They love the errors and superstitions of their forefathers. They are encouraged by the enthusiasm, the loudness, the richness of resource, the unfaltering faith of the famous, busy, mere "practical man." He and they have always numbers on their side. Disease is always rife and deadly, yet the "practical man" always satisfies himself and the public that he is successful, curing and discovering new cures. Such men take little trouble about diagnosis; they leave that to the slow scientific man; their business is to cure, not to follow the scientific method of first ascertaining what is the disease, and what are its circumstances, whether it is curable or not. Your practical man is above all that: he can even consult and prescribe by telegraph in an acute case.[1]

A clever Chinese doctor, formerly a pupil of my own, now a practitioner in Canton, ingenuously describes to me, with expression of astonishment, the persistent and unalterable preference, on the part of his fellow-countrymen, of their own old-fashioned medical treatment to that which he wished to introduce from Edinburgh. There was little difficulty about the introduction of improved surgery and midwifery, but they would none of our ordinary medical treatment. There is no doubt that the Chinese are distinguished among the nations for a morbid spirit of conservatism, and I dare say there is as little doubt that our general and special medical intelligence is much higher than that of the Chinese. It is on these accounts probable that our modes of treatment are, on the whole, better than theirs, somewhat more in accord with the general tenor of pathology. But, apart from these considerations, what can we urge to demonstrate the supposed backwardness and folly of the Chinese? Literally nothing. In fact, there is scarcely a pin to choose between us. How lamentable a position is it for us, in 1873, to be unable to give chapter and verse in proof of the advantages of our treatments! Indeed, were a judge between us disposed to be sarcastic, and justly so, he might taunt us with our ordinary proceedings, having, for the most part, no better foundation than old wives' fables, than homœopathic vanities, than grossly false superstitions in religion. He might say—Where is your evidence for the value of your former system of bleeding in most diseases, even in hæmorrhages? Where is your evidence for the value of your abstinence from bleeding now? Where the evidence of the value of your present system of feeding and filling with alcohol? Where is the evidence for the value of horse-riding used formerly, or for the value of rest used now? Where is the evidence for the value of your former treatment by mercury in a host of diseases? Where the evidence of the value of abstaining from it? I fear that, if called to give scientific answer to these questions, and many such, we could only be dumb, and recognise in the Chinaman a wisdom in his ordinary medical treatment that may be equal to that of our own.

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[1] See the Appendix.

In the absence of scientific evidence, who can arrive at a satisfactory decision? Both the Chinaman and the Scotsman would appeal to some modification of the argument from "semper, ubique, et ab omnibus," or to the "verba magistri;" but, in science, such evidence goes for nothing: it is not only unsatisfactory, it is worthless. Medical science, as pure science, is very far in advance of its ancient position; but medical practice, or medicine proper, so far as it is not scientific—and here we include almost the whole of ordinary medical practice—is just where it was in the days of Dioscorides and of Galen.

The scientific practitioner, if such a being be as yet a possibility, is he who can tell why he has faith in his remedies so as to satisfy every one capable of judging. He must not lean on authority, or on ancient usage, or on widely-prevalent assertions of success. In the lamentable deficiency of scientific medicine, the practitioner is constrained to lean on authority, and other such frail supports; but it is supremely desirable that he should know and recognise what he is trusting in. These frail supports maintain the whole armamentarium of the mere practical as distinguished from the scientific practical physician—of the "practical man" so highly valued by the unscientific mass of the profession and by the general public, the authors of fine practical papers so eagerly sought after by the less instructed, so frequently despised by all the rest.

It must be distinctly admitted that, while all this armamentarium of the merely practical man will ultimately be laid aside, and while advancing science alone can hasten the advent of the desired time, yet, at present, the honest practitioner, imbued with the scientific spirit, may honourably resort to such means. He will do so as seldom as possible, and always with a feeling allied to shame; but he may regard himself as justly compelled by consideration of the very little grain of truth that lies in the proverb, "Anceps remedium melius quam nullum," and by the generous desire to innocently gratify the weakness or ignorance of the patient, who will draw much comfort from the use of the trusted drugs.

It would be equally difficult and useless to name the very numerous drugs and appliances of the "philosophical practical man." They fill our so-called pharmacopœia, and are stored in druggists' shops. Your mere "practical man" has no difficulty in fixing upon the right one, in the right dose, at the right time;

being at the same time as ignorant scientifically on these points as a mole is blind. These drugs used formerly to be derived chiefly from the vegetable world, and constitute the lore of Dioscorides and other authors, down to the authors on materia medica of our own day. The latter have, however, now to adopt another mass of equally valuable jargon—a host of discordant names of compounds revealed to us by the skill and science of the modern chemist. Nearly the entire belief in them and use of them is founded on ignorance. The rise and fall and resuscitation of such remedies as bleeding, and mercury, and antimony, and countless others—some of them regarded as blessings to the race, and their discoverers rewarded by the State—is humiliating evidence of the truth of what I say. The charms and philtres and incantations of old days are translated into the drugs and appliances of our time. There was the same evidence for the utility of bleeding, of dragons' blood, or of the powdered bones of a murderer, or of a bit of calcined placenta, as there is now for the remedies of the mere "practical man." He is the priest of a false religion, superstitious himself, and working on the superstition of his clients. He has all the varied sources of power and of a kind of usefulness that the honest priest of a false religion has, and no more. How many or how few drugs are there in the British Pharmacopœia of whose utility there is a tittle of scientific evidence?

I have already said that the difficulties in the way of subjecting all departments of medicine to scientific method are very great. For the history of the subject, and some natural prejudices, make it almost painful to reject the old philosophical treatment of it. We appear to many to be almost, if not altogether, impious, if, in the spirit of modern science, we decline to admit the existence of the vital principle, or any such power or force, under whatever hallowed name it may be identified, for whose existence there is no scientific evidence, and against the probability of whose existence there is very much. Yet we must decline to take refuge in any such asylum of ignorance, and do our best to make it a hornet's nest for any philosopher disposed to flee to it. In like manner, we appear rude if we reject the *vis medicatrix naturæ*, which should be the deity of expectants. To trust in such an imagined force, such a helpmate for the spurious philosophers, would be ridiculous in a scientific man. He would be resting on a *vox et præterea nihil.* To say that a wound is healed by the *vis medicatrix*, or any disorder righted by the same, would be equivalent to, and even more ridiculous than, saying that a watch was repaired by the watchmaker's fee. All such difficulties as are involved in the giving up these fine phrases, which have long kept the profession employed in beating the air, must be cheerfully met. We must learn to believe in an ignorance more honourable than pretended and false science, and to say with humility and boldness, we do not know; and the acknowledgment of ignorance will be the first step towards replacing the ghost of a name by the substance of real knowledge.

Between our present insecure position and a sound scientific basis of medicine, the gap seems almost infinite, to be beyond our power to bridge over. The extreme complexity of the phenomena, the imperfection of the auxiliary departments of physics and chemistry, and the interference of the psychological element, seem at present to bar the way of progress. But we may predict that they will not long continue to do so.

As scientific men, we cannot despair when we survey what has been already done. As practical men, meeting daily professional difficulties, we can rejoice not so much in the discovery of cures, as in a usefulness immeasurably increased by the advances of science, especially in the branches of diagnosis and prognosis. How much do mere practitioners owe to Harvey and Laennec? How much to Bright and Christison, and a crowd of followers? How much to William Hunter, to Solayres, and to Naegele? How wonderful, almost marvellous, is the utility which science has conferred on the ophthalmologist! We do not despair, but, stimulated by just pride in past achievements, we aim at further progress. How much may we anticipate from the discovery of the mode of propagation of such diseases as cholera and typhoid fever! How much are we realizing as well as expecting from the researches now advancing into the theory of all the septic diseases once so hopelessly obscure!

It is by following scientific routes that we may expect to increase that knowledge which is the best converger of beneficent power. No doubt, some great and immediately useful discoveries are made in the way of lucky hits, and at once place the names of those connected with them in an envied empyrean of fame; but how very very few they are! How slavelike is the labour, if any, expended in hunting for them! If any one doubts the almost infinitely superior prolificness of the scientific method over the pseudo-philosophical, or over that of lucky hits, let him dispel his dubiety by reflecting on the discovery and progressively increasing utility of gunpowder, of arms of precision, of steam power, of telegraphy, of photography, of new chemical compounds, and of many others; and I would add, that if he be not convinced, I would condemn him only to a course of reading in the philosophical writings of the great and justly honoured, but fortunately now old-fashioned, medical writers, or in those of the undeservedly honoured of our own day. There are some learned beings so constituted as to find pleasant this work, which is a slow torture to all the rest of the world.

It is a fault of our societies that these views do not sufficiently govern their proceedings, and the result is a great waste of valuable time, ingenuity, and labour. Let us reflect for a moment on the vast piles of learning that have accumulated and are still accumulating in medicine, misdirected work, and think with sorrow on how much that is good and substantial might have been produced by the same efforts had they been well directed. The chief rule of good direction is to shun in the meantime the so-called philosophers, and go over to the side of the scientific men. Medicine is to be built up on a basis not of far-fetched principles, or theories, or systems, but on a basis of physical, chemical, physiological knowledge—not on thoughts, or views, or dreams, but on facts. Without a sufficient basis of fact we should decline to build at all, preferring the serene atmosphere of vacuity to delusive castles in the air.

In medicine, hotly hastening after new cures—useless and contemptible as they generally, though scarcely invariably, turn out to be—it seems to me that we forget two great abiding truths, the resting-places of the scientific man, and the best guiding principles of the practical man. These are, firstly, the old saying that "knowledge is power"—truly power—useful power for the bedside practitioner; and, secondly, the well-known truth, fully demonstrated in recent history, that the progress of science is the best, almost the only, source of progress in the useful arts.

In the view which I now explain, and which I have always taken of this important subject, I am not singular. The same difference between the scientific men and the combined philosophic and practical men is found in the great medical schools of the Continent. There I have remarked that the men known and honoured among us are all, or in a very preponderating proportion, arrayed on the scientific side; while the local celebrities remain on the other, keeping to the comfortable and the best-paying position, level with the ignorance and prejudices of the mass of the profession and of the public. Even in our more purely scientific associations the same excessive tendency to seek practical results prematurely or by a wrong method—to follow in medicine the footsteps, not of Harvey, Laennec, the Hunters, or of Bright or of Goodsir, but to run with the crowd of ephemeral propounders of new cures—has been observed by distinguished philosophers of the right kind. Writing to Faraday, Liebig makes the following remark on his visit to the meeting of the British Association at York:—"What struck me most," says he, "in England was the perception that only those works which have a practical tendency awake attention and command respect; while the purely scientific, which possess far greater merit, are almost unknown. And yet the latter are the proper and true source from which the others flow. Practice alone can never lead to the discovery of a truth or a principle. In Germany it is quite the contrary. Here, in the eyes of scientific men, no value, or at least but a trifling one, is placed on the practical results. The enrichment of science is alone considered worthy of attention. I do not mean to say that this is better; for both nations the golden medium would certainly be a real good fortune." These words very closely apply to our case. Scientific progress is the proper and true source from which practical results flow.

Before passing from this immediate point, I must cite another witness—himself a glorious hero of scientific progress, a medical brother, a grand improver of practical medicine—whose new and thoroughly scientific inventions are now in daily use by us all. Helmholtz makes the following solemn statement, which I might adopt as the text of my whole harangue, which I would hang up, printed in large characters, above my presidential chair, as the best motto in the meantime for this Society's hall:—

> "Wer bei der Verfolgung der Wissenschaften nach unmittelbaren praktischen Nutzen jagt, kann ziemlich sicher sein, dass er vergebens jagen wird."

This deficiency or imperfection of appreciation of the value of science and of scientific method in medicine, easily accounted for as it is, if we take even the most superficial view of medical history, is also seen to be acting and counteracting injuriously in indirect ways—to be the cause of evils which in their turn tend to foster it. I shall only very lightly touch upon one or two of these things, not with any view to their exposure, but that they may be known and recognised, believing that this recognition by our medical brethren will be enough to ensure disapproval, and improvement on the part of all those offenders who are possessed of due intelligence, and actuated by honourable motives.

Were medical science and scientific method duly appreciated, there could not be that weak exuberance of tufthunting and notoriety-hunting which we see so prevalent around us, and in many ways exerting injurious influences. We may blush to read the reminiscences and biographies of some of our most eminent doctors, and to hear their unqualified praise. Such works as we refer to may be very good in the eyes of a circulating librarian, or of a moralist, or of a littérateur, but they are properly looked on as lives of supposed great medical men. And though the men were far better than they are portrayed, they nevertheless do depict themselves with their own hands as luxuriously living on the crumbs of attention shown to them by people really great, or, more frequently, of mere position, carefully noting down, for the observation of the admiring world, that some lord did actually take tea and toast at their breakfast-table, or that they did not quite die of the joy of sitting at dinner with a company of aristocrats. No doubt these dignities have their own attractions, their proper place, and great power, especially in an old community like ours; but the recording of them, the repeated dwelling on them, their manifest paramount position, ought not to be. Yet so it is; and the lamentable state of matters is farther aggravated and diffused by the action of a part of the medical press. There we may, week after week, see carefully recorded, to suit the vulgar taste, how Lord Foxhunt or Lady Kidglove had some disease or some accident, which, in all its generally grandiose insignificance, is, in defiance of propriety, named and described; also how Mr Blackdose (and here is the rub) was in daily attendance, while the great Sir Fly Blister was telegraphed for from the neighbouring metropolis,—bits of news which are appropriate matter for the *Journal of Fashion*, the *Morning Post*, or the *Times*, but which, in a medical periodical, indicate and cause degradation.

As closely connected with this subject, we might discuss the important and difficult questions regarding the conferring of medical baronetcies; and I hope we may even now be justified in considering most of these as not mere matters of Court favour, but as merging in the greater subject of scientific baronetcies, upon which there have already been, and are now, wide differences of opinion among the politicians who regulate the giving, and the scientific men who regulate the accepting, of these titles of honour. Baronetcies are given to medical men for many reasons besides eminence in science; and to such distinctions I here make no reference whatever. The granting of scientific baronetcies is a matter of very great delicacy, responsibility, and importance, as, I presume, they are given with a view to reward the most worthy, and to promote the interests of science. I decline, however, to express at present any opinion on the matter, because it is one of very great complexity and difficulty,—a topic for philosophers,—and because my own views are not sufficiently matured. But I am sure all will join cordially in approving the sentiments of respect for medicine which actuate those surrounding the fountain of honour, who assist in guiding the dispensation of the coveted titles; and most will probably agree that there can be no great mistake in accepting such dignities when we weigh the character and attainments of those who have worthily borne them.

Medical men and medical journals may, 'tis sad to say, recognise more clearly their faults by the light of comparative criticism. For this purpose, let us take engineers and engineering. Here we have scientific men and practical men of all degrees, and a number of scientific and practical journals, also biographies and reminiscences. The lives of Watt, of Stephenson, and of Faraday, three great contributors to the wealth and power of the human race, show clearly where their hearts lay, what they adopted as their highest pleasure. We read of the laborious, joyful pursuit of special knowledge. They do not decorate themselves with feathers and paint, with ribbons and gold, but leave that work to others who may be honourably and imperatively called to do it; themselves yet either absolutely declining some of the distinctions, or showing their distaste for some of the honours, which it may be their duty, nevertheless, to receive and to wear. My acquaintance with engineering journals is neither extensive nor minute; but I have never seen in their pages, nor in extracts from them, such injudicious statements as I have been alluding to in our own journals,—that this engineer was consulted by this duke, or that engineer by another. The work done is regarded, not its unimportant circumstances. A work is not great because it is done for a person of distinction; nor is a case of importance in a medical sense because a great person is the subject of it. We should elevate on a pedestal and admire the successful practitioner rather than the fashionable one.

These last words—the successful rather than the fashionable practitioner—are, however, well fitted to induce a pause in the career of our statements, and to make us consider whether or not they are too plausible. The pause is required in order that we may admit and state the great difficulties that lie in the way of the public or the profession estimating comparative successes. These arise from the want of the means of making a good judgment. The work of an engineer, or of a lawyer, is before the world, and its results are apparent; but it is far otherwise with the doctor: nor can we in the meantime see any good way of remedying the evil. Probably the best is to estimate the intelligence and scientific character of the practitioner; but this, even if well attained, would completely omit many of the important factors of substantial success. To escape from these difficulties, there is a great tendency to place faith in fashionable connexion and other outward tokens of prosperity; and perhaps this mode of judging is better than none. If it is so, we can only lament our unfortunate position, and strive to get for our profession a more scientific basis, whose light-giving quality will dispel many errors in judgment as to real professional eminence.

There can be no doubt that a sound method of judging professional eminence would be almost if not altogether a panacea for our professional failings; and that such a method is utterly unattainable until our profession has acquired a very much higher scientific position; that any improvement on the present methods will be of incalculable value, and will indirectly hasten the period when our profession, being scientific, will have before it a clearer course of progress, and an ever-widening sphere of usefulness.

The profession of medicine, having among its objects the maintenance of health and the curing of disease, is essentially a grand, noble, and beneficent profession, if its cultivators are honourable men. These qualities, however, it has, independently of its cultivators. It sheds their lustre over its cultivators, who ought to be thereby stimulated to do something in return for the glorious heritage which they get without purchase, the best return being the advancement of the science of medicine. But, in so doing, let it be their constant object to attain that ever-present sense of the true dignity of science which renders all its genuine disciples humble in proportion as they keenly feel it; and of its exceeding great rewards, which only steady and honest labour can ever hope to attain.

The great Psalmist says, "The heavens declare the glory of God and the firmament showeth His handiwork." We, as members of this Medical Society, say,—So also does the disordered and diseased human frame. But, alas, the process of this medical demonstration is, as yet, very far from being nearly completed, or the suggested simile fit for being used by a psalmist.

Since the time of David, science has made great progress, and in no department more than that of astronomy, the model science. Were the inspired sweet singer a scientific man of our day, and writing now, he would probably indite the same words as we have quoted. Certainly he could not, even yet, appropriately invoke the diseased human frame to point his moral. It is for us to push forward, to emulate the progress of astronomy, to make our science attract the honourable mention of poets, to make it shine "up with the best," to ennoble the practice of our art by an ever-increasing utility, to dispel the weariness of practice by making it less and less a trade, more and more a scientific profession.

FLOREAT RES MEDICA.

APPENDIX.

I am indebted to Professor Sanders for pointing out to me the following passage in the preface to the second edition of Laennec's *Treatise on Mediate Auscultation*, etc., as an illustration of the subject of my address. I print the whole portion, on account of its many interesting aspects, but especially because it expresses dramatically and excellently much that I wish to impress on the profession. Laennec and Broussais, the scientific practical man and the philosophical practical man, are opposed to one another. Each states his case. Broussais, then at the height of his fame, glorying in his prosperity, spins his theories and boasts of his cures. Laennec, not as yet nearly at the height of his fame, cannot see his antagonist's cures, nor recognise the value of his theories, but works diligently at diagnosis and pathological anatomy; while Broussais rides in his chariot triumphantly. Laennec knew well the attractiveness of Broussais's methods. "Facilis enim," says he, "descensus Averni, suaviusque juvenum mentibus arrident probabiles de causis et naturâ rerum ratiocinationes, quàm sedula ægrotantium observatio, multa lectio, morborumque tediosa in mortuorum visceribus exploratio."

The following is the passage from Laennec's preface:—

"M. Broussais a dirigé contre mon ouvrage des attaques d'une nature toute différente. Il ne conteste point l'utilité de l'auscultation, il reconnaît même avoir vérifié l'exactitude et l'utilité de la plupart des signes dont elle a enrichi le diagnostic médical. Ce sont mes recherches d'anatomie pathologique qui lui ont déplu. Je n'entreprendrai point de répondre en détail à tous les reproches répétés sous diverses formes, dans les quatre-vingts pages de son examen, qu'il a consacrées à la critique de mon ouvrage, ou de quelques opuscules que j'ai publiés antérieurement. Je résumerai seulement ses argumens, et je joindrai en note quelques courtes réponses que je crois suffisantes. Je pense, au reste, que personne ne trouvera qu'il fût difficile d'y répondre plus longuement.

"'1°. *Les altérations pathologiques considérées en elles-mêmes......sont des faits de pure curiosité, et ne sont d'aucune utilité pour celui qui les étudie'* (*Examen des Doctrines médicales*, etc., 1821, t. ii., pag. 674), '*parce que l'inflammation est la cause de ces altérations* (pag. 676), *ou qu'elles ont une autre cause quelconque* (pag. 677), *et qu'on ne doit s'occuper que de pathologie physiologique*' (pag. 679).[1]

"2°. La manière dont M. Laennec envisage et décrit les productions accidentelles *rentre dans les principes du fatalisme* (p. 683).[2]

"3°. M. Broussais m'attribue cette opinion, que les *squirrhes germent spontanément*, parce que j'avoue mon ignorance relativement aux causes premières qui peuvent les produire: et à cette ignorance, il oppose la science certaine qui lui fait voir comment l'*irritation* produit toutes ces altérations diverses

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[1] Je ne puis trouver aucun rapport entre la proposition et ses preuves, lors même qu'elles n'auraient pas besoin elles-mêmes d'être prouvées.

[2] Je pense, il est vrai, qu'il y a beaucoup de maladies que nous ne savons ni prévenir ni guérir, au moins d'une manière certaine et incontestable. Il ne s'agit pas, ce me semble, de savoir si cela est triste; il s'agit de savoir si cela est vrai.

(pag. 697 *et alibi passim*),[1] et les succès nombreux qu'il obtient en cherchant à la prévenir (pag. 686, 700 *et alibi passim*).[2]

"4°. Il me reproche d'avoir *tranché du devin* (p. 723). '*Il* (M. Laennec) *affirme tout cela avec la plus étonnante intrépidité. Il semble qu'il* ait été *dans l'intérieur du corps de ses malades au moment où cette matière a paru d'abord sous l'état crû, qu'il l'a vue croître, envahir les tissus*,' etc. (pag. 733).[3]

"5°. Il me reproche d'avoir fait connaître l'emphysème du poumon (pag. 728), parce qu'il croit pouvoir en expliquer la cause par l'*irritation*. Quant aux mélanoses, il était fort inutile d'en donner la description; il n'y a non plus rien de solide à cet égard que l'explication que donne M. Broussais de leur formation par l'*irritation*. '*Voilà ce qu'il importe au médecin de savoir, et tout ce que M. Laennec a écrit sur ces cancers noirs est purement imaginaire et se réduit à un sombre roman dont j'ai eu beaucoup de peine à terminer la lecture.*'[4]

"Il en est de même des encéphaloïdes (pag. 734), du pneumothorax (pag. 740), de l'œdème du poumon (pag. 742), de l'apoplexie pulmonaire (pag. 745): ces cas divers n'étaient nullement bons à connaître parce qu'ils dépendent tous, suivant M. Broussais, de l'*irritation*.

"6°. M. Broussais me reproche surtout d'avoir tâché de grouper les symptômes des maladies autour des lésions dont ils dépendent, d'avoir cherché à déterminer les modes de lésions qui peuvent exister (pag. 750), chez un malade vivant,[5] et me prédit que je n'atteindrai jamais ce but (pag. 754); de presenter une surabondance de détails anatomiques et séméiotiques fort ennuyeux, propres à décourager le lecteur (pag. 736 et 737).[6]

"7°. Il est *désespéré* de la longueur de mes observations, que la mémoire ne peut retenir,[7] qui présentent toujours, dit-on, une *combinaison de lésions cadavériques*, de sorte qu'on voit quelquefois quatre ou cinq altérations diverses chez le même sujet (pag. 760).[8] Il me reproche enfin de donner trop d'importance à la description anatomique des lésions (p. 711).

"Que répondre en somme à de semblables argumens? Le seul fait qui me paraît en résulter évidemment est celui-ci, c'est que M. Broussais et moi cultivons des sciences tout-à-fait différentes, sinon dans leur but définitif, au moins dans leur objet immédiat. Le but que je me suis constamment proposé dans mes études et recherches a été la solution des trois problêmes suivans: 1°. distinguer sur le cadavre un cas pathologique, aux caractères physiques que présente l'altération des organes; 2°. le reconnaître sur le vivant, à des *signes* certains, et autant que possible physiques et indépendans des symptômes, c'est-à-dire du trouble variable des actions vitales qui l'accompagnent; 3°. combattre la maladie par les moyens que l'expérience a montré être les plus efficaces: en un mot, j'ai tâché de mettre, sous le rapport du diagnostic, les lésions organiques internes sur la même ligne que les maladies chirurgicales, et j'ose croire que tous les médecins instruits qui voudront se donner la peine de vérifier les faits contenus dans cet ouvrage, trouveront que j'ai atteint mon but pour un assez grand nombre de cas. M. Broussais, au contraire, s'est élevé à la recherche des causes prochaines; il méprise les détails minutieux de l'observation, la distinction des cas, et implicitement même la sûreté du diagnostic: car il raisonne toujours dans l'hypothèse qu'il est inutile de distinguer les uns des autres tous les cas auxquels il attribue une cause semblable; et il attribue la plupart des maladies à une seule cause, l'*irritation*.

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[1] Et si M. Broussais était comme les autres hommes sujet à l'erreur?

[2] On verra, dans la suite de cet ouvrage, des relevés statistiques déjà publiés dans divers journaux de médecine et qui n'ont pas été contredits, au moins d'une manière valable à mon avis. On pourra juger que M. Broussais s'est trompé au moins sous ce rapport.

[3] M. Broussais croit-il que le naturaliste qui a trouvé sur le même buisson la larve, la nymphe et le papillon dans leurs divers degrés de développement, ait besoin, pour décrire les métamorphoses de cet insecte, de s'enfermer dans l'œuf ou dans la chrysalide? Pense-t-il que Hunter, Meckel, Tillemann et Pander, soient rentrés dans le sein de leurs mères pour étudier le développement du fœtus?

[4] Je conviendrai volontiers que cela n'est ni long à apprendre, ni difficile à retenir, et que la lecture de mon ouvrage demande plus d'attention et de travail. Je remarquerai seulement que mes *cancers noirs* sont des corps dont les dimensions, la consistance et toutes les qualités physiques peuvent être appréciées par les sens; et que l'*irritation* de M. B......, qu'il définit (passim) *exaltation des propriétés vitales*, est un pur *être de raison*, et pour la plupart des cas où il l'applique, une hypothèse bien peu probable.

[5] Je n'ai fait en cela que suivre la marche universellement adoptée depuis Hippocrate jusqu'à nous pour toutes les lésions organiques, dites chirurgicales, et je ne sais encore quelle limite la distingue des cas semblables qui surviennent dans l'intérieur de nos organes.

[6] Je n'ai pas eu la prétention de faire un livre récréatif, et que l'on pût suffisamment connaître en le parcourant avec une attention médiocre; mais j'espère qu'on en pourra tirer quelque fruit en vérifiant les signes auprès du lit des malades et les faits anatomiques sur les cadavres.

[7] Je n'ai jamais pensé qu'on dût les apprendre par cœur.

[8] Il est vrai qu'en pareil cas je ne sais rien supprimer.

"M. Broussais n'a répondu d'aillieurs à aucune des objections qu'à diverses occasions j'ai été amené à faire contre sa théorie; il a préféré attaquer mes recherches anatomiques dans leur fond même et leur objet. Je désire, pour l'intérêt de l'humanité, qu'il ait raison; que la science crée par ses inspirations dispense les élèves de l'acquisition de celle que nous ne pouvons leur transmettre qu'autant qu'ils veuillent étudier et vérifier avec patience les observations des hommes qui sont venus avant eux; enfin que, comme M. Broussais lui-même, ils puissent, à l'aide des sangsues et de la diète exténuante, guérir ou prévenir toutes les maladies que nous regardons comme incurables, ou dont nous ne connaissons encore aucun moyen préservatif certain (*Ex. des doctrines*, t. ii. pag. 686 et 700, et *alibi passim*). Il est cependant un reproche auquel je ne puis me dispenser de répondre. M. Broussais m'accuse d'avoir puisé dans ses ouvrages sans le citer........., de ne l'avoir cite que pour le blâmer (pag. 714); '*cette méthode*, ajoute-t-il, *est usée, et je crois, sans attaquer la moralité sociale de M. Laennec, la pouvoir qualifier de mauvaise foi littéraire.*' (Ibid.)

"J'ai cherché avec soin à quels articles de mon ouvrage pouvaient se rapporter *ces traits de mauvaise foi ou ces larcins scientifiques*; au reste, M. Broussais m'a abrégé cette peine, puisque, dans d'autres paragraphes du même article, il a indiqué lui-même à quoi se rapportent ces reproches. Il prétend que j'aurais dû citer de lui deux exemples de gangrène de la plèvre (pag. 725), et deux autres de tumeurs développées dans la plèvre: ce reproche serait très-juste, si j'avais entrepris de donner un catalogue de tous les cas de ce genre bien ou mal observés jusqu'ici. Mais je me suis contenté de rapporter ou de citer un nombre de faits suffisans pour prouver ce que j'avançais, et je les ai choisis parmi ceux qui m'ont paru le mieux décrits. Sous ce rapport, ma manière de voir diffère tellement de celle de M. Broussais, que lors même que j'aurais connu les observations dont il parle, la brièveté et l'incertitude des détails m'auraient empêché d'en faire aucun usage.

"M. Broussais revendique en outre la découverte suivante: 'les glandes bronchiques se teignent insensiblement par les progrès de l'âge......Depuis *que j'ai fait faire cette observation, M. Laennec a voulu distinguer cette coloration naturelle, effet de l'âge, et à la quelle il n'avait pas d'abord songé, d'avec ses mélanoses*' (pag. 701). Ce n'est pas à moi que doit s'adresser cette revendication, mais bien à Fourcroy, qui a analysé la matière noire des glandes bronchiques, à une époque où M. Broussais n'était probablement pas encore sur les bancs de l'école, et à tous les anatomistes des deux derniers siècles qui en ont parlé.

"Enfin, M. Broussais assure avoir dit avant moi '*qu'une foule de personnes sont douées d'un cœur trop volumineux* (p. 751), *relativement aux autres organes* ...., *et peuvent parcourir cependant une longue carrière sans devenir anévrysmatiques.*' J'ai dit, il est vrai, l'équivalent de ce qu'on vient de lire, et je l'ai dit parce que je l'ai vu. Je ne nie pas que M. Broussais ait pu reconnaître le même fait et en parler avant moi. J'aime mieux le croire que de lire des ouvrages dont la nature ne comporte aucun plan régulier, avec l'attention qu'il faudrait pour y trouver une remarque aussi simple, et que tous les praticiens ont sans doute faite comme nous.

"M. Broussais lui-même trouverait-il juste les revendications de ce genre, que beaucoup d'auteurs vivans et morts pourraient faire sur ce qu'il a publié et même sur les 468 axiômes sur lesquels il appuie *sa doctrine.*[1]

"Je terminerai avec M. Broussais comme il termine avec moi, par quelques conseils en échange de ceux qu'il a bien voulu me donner, et dont malheureusement je n'ai pu faire mon profit. Ainsi, je ne puis me déterminer à *suivre ses exemples*, quoiqu'il m'en ait sommé par trois fois. Je ne puis non plus me déterminer à *faire quelques sacrifices* A L'AMOUR PROPRE, fussé-je certain par-là de devenir, comme il me l'assure, *un médecin physiologistes des plus distingués* (p. 750). Il n'en a lui-même que trop fait, à mon avis: l'amour-propre n'est bon à rien qu'à étouffer la vérité et à éterniser les discussions. Je lui conseille plutôt d'abandonner ce ton de supériorité qui sied peu quand on parle à ses pairs, ces expressions figurées ou polémiques peu propres à convaincre des esprits refroidis par la culture sérieuse des sciences physiques; d'attacher moins d'importance à des mots qui n'ont de valeur et de sens que celui qu'on leur donne par une bonne définition; de chercher un juste milieu entre mes longues descriptions anatomiques et ses courtes observations; de ne pas prendre des objections pour des concessions;[2] de substituer à la dénégation des faits qu'il ne connaît pas, le silence ou le simple doute philosophique; de ne rien réclamer sans être bien sûr de sa propriété, et de négliger même ce qui ne lui appartient que parce que cela appartient à tout le monde; et alors je crois, comme lui, que nos manières de voir commenceront à se rapprocher. Alors il nous accordera sans doute cette proposition qui ressort des ouvrages et des exemples de tous les princes de la médecine, depuis Hippocrate jusqu'à notre temps, et que Bâcon a si heureusement exprimée en ces termes: '*ars medica tota in observationibus;*' et de notre côté, nous conviendrons volontiers qu'il est dans la nature de l'homme de chercher à lier entre eux les faits dont l'ensemble constitue une science, que l'étude des anciennes théories, les efforts pour en créer de nouvelles, peuvent être loués comme des amusemens de l'esprit, pourvu qu'ils ne servent qu'à rallier les faits, et qu'on soit prêt à les abandonner dès qu'un fait leur résiste; mais nous penserons toujours que, si les propriétés des corps et les actions qu'ils exercent les uns sur les autres, objets de la physique et de la chimie, ne peuvent être étudiés sous un même point de vue théorique; que, si dans l'état actuel de la science, on ne peut rallier les faits physiques et chimiques que par groupes et à l'aide de théories diverses, qui n'ont le plus souvent aucun rapport entre elles; à plus forte raison les maladies, modifications dans l'économie animale, qui peuvent être produites par tous les agens physiques et chimiques, et de plus par des aberrations des actions vitales, pour l'étude et l'appréciation desquelles nous n'avons presque aucuns moyens, ne peuvent pas être rapportées seulement à deux causes opposées."

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[1] Je n'en citerai qu'un exemple. "Lorsque l'idée m'est venue de combattre la colite (la dysenterie) par des sangsues placées à l'anus, je ne connaissais aucun exemple de cette pra-

tique" (*Annales*, t. ii., pag. 179). M. Broussais croirait-il pour cela, que personne ne doive plus parler, sans le citer, de ce moyen thérapeutique. Il faudrait alors que, pour remplir toute justice, il n'en parlât pas lui-même sans citer au moins les auteurs suivans, vivans ou récemment morts, et sans doute beaucoup de praticiens de divers pays:

HUNNIUS, *von der Ruhr*, pag. 133; HEUMANN, *in Baldinger's Magazin*, vol. xx. pag. 102 et 121. *Voy.* PLOCQUET, *Litterat. med. dig.* Tubingæ, 1808, tom. i. p. 429.—ZIMMERMANN, *de la Dysenterie*, 2e édit. de la traduction française, 1794, p. 271.—PINEL, *Nosogr. Philos.*, t. ii. p. 335.—J.-A. FLEURY, *Dissert. sur la Dysenterie*, an xi (1803).—P. POUMIER, *Dissert. sur la Dysenterie*, 1804, et plusieurs autres thèses du même temps, de la Faculté de Paris.

Que dirait M. Broussais si quelque disciple du docteur Vitet, auteur de l'ouvrage intitulé: *Médecine Expectante*, 6 vol. in-8° Lyon, 1823, l'accusait de plagiat, pour avoir, après son maître et beaucoup d'autres praticiens plus obscurs, donné une préférence presque exclusive aux sangsues sur la lancette; si ses condisciples et les practiciens plus anciens de Paris lui disaient qu'avant lui, Corvisart et beaucoup d'autres médecins antérieurs appliquaient des sangsues dans les fièvres continues, suivant la nature des symptômes, aux tempes, sur l'épigastre ou la région iliaque; qu'ils savaient poursuivre par le même moyen, dans toutes les maladies, les douleurs locales qui leur semblaient de quelque importance; que cela s'appelait *faire la médecine du symptôme;* que l'usage des sangsues était surtout devenu très-fréquent depuis la révolution, à raison de la difficulté de trouver des chirurgiens qui voulussent faire une saignée; que depuis Hippocrate tous les médecins prescrivent une diète sévère et des boissons délayantes dans les maladies aiguës; qu'enfin le burlesque portrait du docteur Sangrado n'est que la copie fidèle d'originaux existans dans le dernier et l'avant-dernier siècle, et que par conséquent toutes les fois qu'il parle de l'emploi de quelqu'un de ces moyens, sans citer personne, il se rend coupable de plagiat. M. Broussais trouverait-il ces reproches justes et raisonnables?

[2] *Voyez* comparativement l'*Auscultation méd.*, 1e édit., tom. ii., pag. 320, et le Nouvel Exam. pag. 758.

ARTICLE II.—*On Idiocy, especially in its Physical Aspects.* By Dr W. W. IRELAND, Scottish National Institution for the Education of Imbecile Children, Larbert, Stirlingshire.

(*Continued from page* 610.)

Microcephalic Idiocy.

As far as my own measurements go, they confirm the conclusion of Esquirol, that the average size of the heads of idiots, excluding those of hydrocephalic ones, is somewhat smaller than the average size of healthy people; but to this rule there are many exceptions. Some have heads larger than the average size of sane individuals of the same age; and I have measured heads of people of normal intelligence, which are smaller than any in the Larbert Institution, with two or three exceptions. Moreover, those idiots who have larger heads do not surpass in intelligence those who have smaller ones. Save in the cases of hydrocephalic and microcephalic idiots, the size of the head gives no estimate of the comparative intelligence of the children.

Seguin gives an able summary of the state of the question in the following passage:[1]—"Gall, giving a strong impulse to the investigation of the functions of the brain, had called up the question of the cause of idiocy: a skilful theorist, he thought he had discovered in idiots proofs of the truth of his system of phrenology. The authors who succeeded him—Georget, Esquirol, Lelut, Foville, Calmeil, Leuret, Pritchard—seem, on the contrary, to have studied idiocy only to use its phenomena for the destruction of the system of Gall, but not for the benefit of the poor idiots whom they declared incurable. With their single polemical object in view, they spent thirty years in measuring and weighing the heads of living and dead idiots, and they arrived at the following conclusions:—

"1. No constant relation exists between the general development of the cranium and the degree of intelligence.

"2. The dimensions of the anterior part of the cranium, and especially of the forehead, are, at least, as great among idiots as among others.

"3. Three-fifths of idiots have larger heads than men of ordinary intelligence.

"4. There is no constant relation between the degree of intelligence and the weight of the brain.

"5. The different degrees of idiocy are not measurable by the weight of the brain.

"6. A cranium, perfectly formed, often encloses a brain imperfectly formed, irregular, etc.

"7. Sometimes the brain of idiots presents no deviation in form, colour, and density from the normal standard; it is, in fact, perfectly normal."

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[1] See Report of the Commissioners on Idiocy to the General Assembly of Connecticut. New Haven, 1856, p. 57.

It is, however, agreed that there is a certain minimum size of head below which the possessor is necessarily an idiot. Voisin says that the proper exercise of the intellectual faculties is impossible with a head of from eleven to thirteen inches in circumference, and a measurement of eight to nine inches from the root of the nose to the posterior border of the occipital bone. To this rule there has never been an exception. He thinks that heads from fourteen inches to seventeen inches in circumference, and from eleven to twelve inches for the arc comprised between the root of the nose and the foramen magnum, are too small for ordinary intelligence; but heads of from eighteen to eighteen and a half inches, though smaller heads, allow of the regular exercise of the intellectual faculties. Mr George Combe, who must have measured a large number of heads, apparently quoting this passage of Voisin,[1] renders it, "heads of eighteen inches round give intellectual manifestations regular, but deficient in intensity." Mr James Straton measured the head of a boy in Aberdeen of eighteen inches circumference, whose brain, by his system of computation, was estimated at 82 cubic inches; "but it was well balanced, the constitutional temperament highly nervous, and the boy is quite as intelligent as could be expected of his age in his circumstances."[2]

I think we may therefore assume that below seventeen inches in circumference the manifestations of intellectual power would be feeble. But heads of this small scale are rare even amongst idiots, for idiocy is generally the result of disease, not of smallness of the brain.

Microcephaly is the rarest of all kinds of idiocy; nevertheless, owing to the speculations of Darwin, microcephalic idiots have been carefully studied, whereas those who possess brains of normal size have met with little attention. Charles Vogt has collected, from all available sources, as many as forty-two cases of microcephales, and his monograph[3] will probably remain the principal work on the subject for a long time to come.

Microcephaly may be either general or partial. Certain portions of the encephalon may be abnormally small or altogether wanting; but in general the deficiency consists in the smallness of the hemisphere. The nerves of special sense are generally well developed, and the ganglia of the base of the skull and the spinal cord are much nearer the normal size than the hemispheres. The cerebellum also is much longer than in the normal brain. In the case of a microcephalic idiot in the asylum at Bareilly, the cerebellum was one-half of the weight of the cerebrum, the ordinary relation being one to eight. In many cases the superior sutures of the cranium are closed. In three heads of microcephales dissected by Gratiolet, the sutures by the base of the skull were behind the usual stage of development, while those above were closed; the inverse condition is often met with in cretins. So many cases have been collected of microcephales with open sutures, that it is not likely any one[1] will continue to hold that the small size of the brain is owing to the sutures closing in and thus hindering its growth. Even in those cases where the sutures have closed in before birth, and which it must be granted are not uncommon, how can it be decided whether the brain ceased to grow because the sutures are closed, or whether the sutures closed in because the brain ceased to grow, or, lastly, whether both the brain and its coverings ceased to grow under the influence of a common cause? It seems to me that if the sutures closed in and the brain continued to grow, the symptoms described as following hypertrophy of the brain would be produced, which I think is not the case.

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[1] I give the sentence in the author's own words :—"Les têtes de dix-huit pouces à dix-huit pouces et demi sont encore de petites têtes quoiqu' elles permettent à l'exercice regulier des facultés intellectuelles." De l'Idiotie chez les Enfants, par Felix Voisin, Médecin en Chef de l'Hospice des Alienés de Bicêtre. Paris, 1843, p. 73.

[2] See Review of Contributions to the Mathematics of Phrenology, by James Straton, Zoist, vol. iii. p. 426.

[3] Mémoires sur les Microcephales ou Hommes-Singes, par Charles Vogt. Genève-Bale, 1867.

M. Cruveilhier[2] speaks of a microcephalic child which lived eighteen months, where the vertical diameter of the cranium measured no more than an inch. "This child," he adds, "never gave any sign of intelligence; its limbs were incessantly in movement; it expressed its wants by cries; all the bones of the cranium were united without any suture." Cruveilhier does not describe the appearance of the brain, which must have been so small. It died of convulsions, to which it was subject from birth. Professor Turner[3] cites the case of an idiot recorded by Theile, where the brain weighed only 10·6 ounces; another case, by Mr Gore, where it weighed 10 ounces and 5 grains; and a third case, from Mr Marshall, where the weight was as low as 8¼ ounces. The heads of the Aztecs were amongst the smallest known. These were two children who were exhibited in America, England, and France, about twenty years ago, with a trumped-up story about their origin. The following description is taken from Dr Dalton's Treatise on Human Physiology:[1]—

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[1] It seems to me that Vogt has misstated the views of Virchow when he says that the German pathologist holds that—"La microcéphalie doit être nécessairement combinée avec les synostoses prédominantes de la voûte cranienne."—P. 88. In reply to this, it is enough to cite Virchow's own words: "Dass es Fälle von Mikrokephalie ohne Synostoses der oberflächlichen Nähte gibt habe ich früher erwähnt."—Untersuchungen über die Entwickelung des Schädel-Grundes, von Rudolf Virchow; Berlin, 1857, s. 80-105. Virchow über Die Entwickelung des Schädel-Grundes, s. 80-105. Rokitansky, Lehrbuch der Pathologischen Anatomie, Zweiter Band, Wien, 1856, p. 433. Dr Westphal, in Proceedings of Berliner Medicinisch-Psychologische Gesellschaft, reported in Archiv für Psychiatrie, iv. Band, 1 Heft; Berlin, 1873, s. 261.

[2] Anatomie Pathologique Générale. Paris, 1856, tome troisième, p. 164.

[3] See his paper on the Convolutions of the Human Brain considered in relation to the Intelligence, in the West Riding Lunatic Reports, vol. iii. p. 10.

"They were boy and girl, aged respectively about seven and five years. The boy was 2 feet 9¾ inches high, and weighed a little over 20 pounds; the girl was 2 feet 5½ inches high, and weighed 17 pounds. Their bodies were tolerably well proportioned, but the cranial cavities were extremely small. The antero-posterior diameter of the boy's head was only 4½ inches; the transverse diameter less than 4 inches. The antero-posterior diameter of the girl's head was 4⅛ inches; the transverse diameter only 3¾ inches.

"The habits of these children, so far as regards feeding and taking care of themselves, are those of children two or three years of age. They were incapable of learning to talk, and could only repeat a few isolated words. Notwithstanding, however, the extremely limited range of their intellectual powers, these children were remarkably vivacious and excitable. While awake, they were in almost constant motion; and any new object or toy presented to them immediately attracted their attention, and evidently awakened a lively curiosity. They were, accordingly, easily influenced by proper management, and understood readily the meaning of those who addressed them, so far as this meaning could be conveyed by gesticulation and the tone of the voice."

Seguin,[2] who saw the Atzecs in America, thought them susceptible of a certain amount of education. "He considered them," says he, "capable of being elevated from the level of the monkey to that of obedient, sensible, and happy children. This prospect, frankly laid out, did not seem to please their keeper, either because he did not seem to believe it possible, or because he feared it would have diminished the chances of his success."

Dr James Murray Lindsay, now of the Derby Asylum, has kindly allowed me to publish the case of a very small brain which he examined. It belonged to an idiot who was admitted into the Hanwell Asylum on the 5th June 1865. She was then eleven years old, and able to walk. The limbs were misshapen with rickets. She was stunted in growth, and of a childish appearance. She was unable to say a single intelligible word, but chirped like a bird. She seemed to pay no attention to sounds, save the jingling of keys or the playing of musical instruments. "She was," writes Dr Lindsay, "not nearly so intelligent as a monkey I have here, or a dog which I had at Hanwell." She died on the 16th April 1871, in the seventeenth year of her age, having been nearly six years in the asylum. "The chest, spine, and limbs, were found greatly deformed. The heart was very small, weighing 2¼ ounces, with a valvular opening near the foramen ovale. The antero-posterior diameter of the cranium was only 4½ inches; the lateral, 3½ inches; measured from ear to ear across the vertex, it was 8¼ inches. The calvaria were very thick and dense, especially in front and near the parietal ridges. The brain weighed no more than 13 ounces. The convolutions were shallow and few in number. The cerebrum was wanting in development posteriorly, and did not overlap the cerebellum. The brain-substance was softened and watery throughout. The ventricles were dilated with serum. The choroid plexuses were œdematous." Thus the brain was not only extremely small, but what was of it was diseased.

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[1] Philadelphia, 1871, p. 439. The book has an engraving of these children.

[2] See Idiocy. New York, 1866, p. 343.

In an Indian Report we have a notice of a brain even smaller than this in the Bareilly Asylum. "During the year," writes Dr J. H. Lock,[1] the superintendent, "one of two microcephalous idiots in the asylum died of dysentery; he was unable to articulate, walked very weakly, with a half-running gait, could only be made to understand about his wants for food and clothing; and was, in fact, very little raised above the condition of one of the lower animals. I am sorry I cannot give his weight, but his height was 4 feet 8 inches, and the weight of his brain—cerebrum, 6¼ ounces; cerebellum, 3¼ ounces; pons and medulla oblongata, ¼ ounce. The convolutions were rather flatter than usual on the lower side of the brain, and the middle lobe was less prominent below than it generally is; otherwise the brain was normal."

Vogt believes microcephaly to be a case of atavism, the appearance of a type of brain inherited from some very remote ancestral ape. As an illustration, he gives the occasional appearance of the two supplemental toes in the horse, which, he thinks, indicates its descent from the hipparion, an extinct animal of the Pleiocene period, which had two shorter toes on each side of the hoof—somewhat like the posterior toes of the deer and ox. This, Vogt observes, disappears teratologically as an arrest of development, for in the embryo horse the representatives of the lateral toes appear, but only to be incorporated at an early date with the single row of metacarpal bones and phalanges. In the same way, the brain of the microcephalic idiot is the result of arrested development of a human brain checked in its evolution at the simian stage.

The observations of Gratiolet are far from confirming this theory. "The study of the brain of microcephales", he writes,[2] "has furnished me with other reasons for proving through anatomy the absolute distinction of man. On comparing attentively the brain of apes with that of men, I found the arrangement of the central convolutions to be in adult age the same in both groups. If one went no further, there would not be sufficient grounds to separate man from animals in general, but the study of development gives us a real distinction. The temporo-sphenoidal convolutions appear first in the brain of the ape, and the frontal lobe last; but exactly the opposite takes place with man—the frontal convolutions appear first, the temporo-sphenoidal last. Thus the same series is repeated in the one case from Alpha to Omega, in the other from Omega to Alpha.

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[1] See General Report, No. 4, on Lunatic Asylums, Vaccination, and Dispensaries in the Bengal Presidency for the year 1871, compiled by Assistant-Surgeon K. M'Leod, A.M., M.D. Calcutta, 1873, p. 25.

[2] Mémoire sur la Microcéphalie considérée dans les Rapports avec la Question des Caractères du Genre Humain, par le Docteur Pierre Gratiolet; Journal de la Physiologie de l'Homme et les Animaux. Paris, 1860, p. 110.

"From this fact, which was rigorously verified, there flows a necessary inference: no arrest of development can make the human brain more nearly resembling that of the ape's than it is in the adult; far from that, it will differ so much the more the less developed it is. This inference is completely justified by the view of the microcephalic brain. At first it might be taken for the brain of some new and unknown ape; but the slightest attention is enough to save one from this error. In the ape the parallel fissure is long and deep, and the sphenoidal lobe is marked by complicated furrows. In the microcephale, on the other hand, the parallel fissure is always incomplete, and sometimes awanting, and the sphenoidal lobe is almost entirely smooth. That is not all: in the microcephale the second bridging convolution, between the parietal and occipital lobes, is always superficial, a character peculiar to man.[1] In the pithecæ, on the contrary, the convolution is constantly hid under the operculum of the occipital lobe. Thus, in the depth of their degradation, the brain of the microcephale presents human characters often less voluminous and less convoluted than those of the ourang or chimpanzee; they do not become similar. The microcephale, however low he may be, is not a beast, but a diminished man.

"I have examined the question, Does microcephaly precede birth? Of this there can be no doubt. In one of the cases of microcephaly which I have studied, the general form of the brain and of the fissure of Sylvius showed that the monstrosity was at least contemporary with the fifth month. It is probable that this state depends upon some cause: early under the influence of some primordial generative weakness (*asthéniogenie primordiale*), forms are produced which differ from all normal states. Moreover, in the new-born child, in its normal condition, the arrangement of the cerebral convolutions is complete in all its parts. If microcephaly were after birth, these convolutions would remain, and the volume of the brain alone would be diminished; but it is not so; the growth has languished from the beginning, its fold is shortened, and has stopped growing too soon."

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[1] Professor Turner is inclined to doubt the correctness of the views of Gratiolet on this point. See his "Notes on the Bridging Convolutions in the Brain of the Chimpanzee," from the Proceedings of the Royal Society of Edinburgh, 1865-66.

Gratiolet remarks that those microcephales in whom the convolutions are so little complicated are all dwarfs.

The brain of Sophie Wyss, of whom there is a detailed account in Vogt's work, was exhibited by Dr Cramer at a meeting of medico-psychologists at Zurich.[1] Its capacity was 360 cc. Two other brains of microcephales were shown at the same time. In all these brains Dr Cramer pointed out the island of Reil was uncovered by the temporal and parietal gyri, while in the monkey the island is always covered.

Dr Cramer had, however, read of microcephalic brains which approached more nearly those of the ape.

In the microcephales the impressions of the senses are lively. They are fond of moving about, but have little power of continuous attention. Their restless motions recall those of the butterfly. It would appear that they are late in learning to walk; but in general they have the free use of their limbs, which Gratiolet accounts for by the comparatively large development of the cerebellum.

If the brain be healthy, the prognosis is better than that of many cases where the brain, though of normal size, is the seat of chronic disease. I have heard of several cases where, though the head was very small, the mental manifestations were greater than one would be disposed to believe; but I am not inclined to repeat details where I cannot furnish measurements more precise than those reproduced from the memory.

Vogt and other disciples of Darwin see in the mental characteristics of the microcephalic idiot something resembling those of the anthropoid ape. It seems to me that the intelligence of a monkey is very different from that of an idiot. You cannot reach the simian intellect merely by deducting so much from the human. It is different in kind as well as in degree. One might as well expect to find the same character of intelligence in an infant of two months old and in a full-grown chimpanzee, because the cranial contents were about the same. The mental powers which the monkey possesses are in perfect accordance with his organism. His agility in climbing and swinging himself from branch to branch is something marvellous. I have seen whole flocks of monkeys running down a wooded hillside with the greatest rapidity, without ever touching the ground, leaping from one branch to another, sometimes laying hold with the superior, sometimes with the inferior extremities, never tumbling, and scarcely ever missing their aim. This demands some kind of mental as well as physical powers, and cannot be clouded over by the vague word instinct. Monkeys, as every one knows, are extremely alert, watchful, and nimble; very careful against wild animals; they do not lie down to sleep, but sit upon trees all night; their slumbers are very light; they are attached to and careful of their young.

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[1] See the Correspondenz-Blatt für Psychiatrie und gerichtliche Psychologie. Februar 1873.

On the contrary, microcephalic idiots, though in general more lively than those of other classes, have no fondness for climbing, and are as destitute of animal instincts as they are of human intelligence. They have no powers either of feeding or protecting themselves from danger, and if left to themselves would soon perish. They present the effaced lineaments of a human being, which only a wandering fancy will mistake for those of an ape. What qualities they have are of a human character. They laugh at what amuses them; they have human sympathies and human affections. Where they learn a few words, they use them as human beings do to signify things. Their tendency to imitation is often strong, and reminds a superficial observer of the monkey; yet imitation is also strong in the human being, especially in children. The resemblance between microcephales and apes seems to me to rest especially upon the negative quality of stupidity. Vogt remarks that many of them do not speak at all; but then it is clear the cases he describes never received that species of training which is required for beings of their low mental capacity. I cannot at present recall more than two cases of microcephalic idiots who were sent to a training school. In both the head was very small. In one who is at present in the Royal Albert Asylum at Lancaster,[1] the dimensions are, or were in 1871—

	Inches.	Centimetres. About
Anterio-posterior,	7⅞	19½
Circumference,	14⅛	36
Transverse,	9⅞	25

This little fellow is thus noticed in the Report for 1872 of the Royal Albert Asylum:—

"F. Y. P., admitted February 1871, aged 7. An active, restless boy, with peculiarly small head. On admission speech was absolutely wanting, and the power of attention was very feeble. Now (September 1872) says after teacher 'P,' 'I,' 'see,' 'me,' 'D,' and 'C,' and is an attentive pupil at object-lessons."

A boy is mentioned by Dr Wilbur, in his Report of the New York State Idiot Asylum for 1857, who had a head of only 13¼ inches in circumference. He was admitted at the age of eleven, and was mute. After a year he learned the names of all the objects in the school-room and about the house, and the names of all the pupils in the school. It may be here noticed that though many idiots are mute, I cannot recall a single instance where an idiot picks up words which he cannot understand, like a parrot or a starling; and they often understand what is said to them years before they commence to speak, and some who understand many words remain mute for life. I know of one small-headed idiot

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[1] See the Journal of Mental Science for July 1872, p. 303, and for October 1872, p. 351.

who seems somewhat dull of hearing; he only uses two words, "me" and "no," but always in a correct sense, whether separately or combined.

Congenital Idiocy.

The phrase congenital or born idiocy is no new one. Idiocy, writes M. Parchappe,[1] depends upon congenital atrophy of the brain; idiotism upon an atrophy following upon a chronic affection. Other French physicians have used the term idiocy to signify a congenital vice; imbecility to denote a disease acquired after birth. It ought to be noted that the term congenital idiocy is here employed in a somewhat different sense to comprehend all those cases which, shrouded in the obscurity of intra-uterine existence, cannot be traced back to any known specific disease. It thus comprehends the cases whose pathology cannot be properly diagnosed till after death. Thus, cases of inflammation of the brain occurring before death are, as far as my knowledge reaches, not distinguishable from other congenital cases. A microcephalic idiot is a born idiot, but I do not include him under the term congenital. In process of time, by carefully studying the symptoms in life and the lesions after death, we may be able to resolve congenital idiocy into some new or old classes. In the meantime, a large number of our cases must still be included under the heading of congenital idiots. At present, if we cannot classify them in a more precise way, we at least may save confusion by putting them aside from the other classes, and inviting attention to the unresolved problems which they represent.

The predisposing causes which have been assigned for congenital idiocy are numerous, but their operation is vaguely understood. Anxiety and fright to the mother during gestation is frequently put down as the original cause. As may be supposed, there is often a hereditary connexion—insanity, epilepsy, or some other nervous disease, is known to exist in the family; and sometimes, too, the tendency has been intensified by a consanguineous marriage. In these cases occasionally the child is born idiotic, and grows up without taking any fits, while another child, born apparently with normal intelligence, inherits a tendency to fits which bring on idiocy in their course. Jastrowitz has indicated hydramnios as a probable cause of congenital idiocy. Professor Betz[2] of Vienna, in a demonstration of the brains of an imbecile and two idiots, has noted the difference of the convolutions. He finds the characteristic distinction in the arrangement of the gray matter, which is different both from the normal human brain and from that of the ape. The bridging convolutions at the base of the external occipital fissure were found deficient in the idiots. In normal human beings the gray substance of the brain is a conjoined mass; in these idiots the connexions were not so close (*auseinander geworfen*).

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[1] Récherches sur l'Encephale, sa Structure, ses Fonctions, et ses Maladies, par M. Parchappe. Paris, 1838, p. 58.

[2] Psychiatrisches Centralblatt, Nr. 7, 25 Juli 1873.

It seems very probable, from the researches of Jastrowitz and other microscopists, that the deficiency in congenital idiocy may consist in the structure of the tissues of the brain, in the persistence of anatomical elements which are normal in the embryo, but which ought to have passed into another form for the mature human being.

A very common accompaniment of congenital idiocy is the keel or saddle-shaped palate. The palate within the alveolar processes resembles the impression made by the keel of a ship, or the inside of a saddle looked at from behind; the jaw is narrow rather than long, and the furrow in the middle becomes deeper towards the back of the hard palate. The teeth are generally bad; about the age of ten or twelve they commence to look black near the margin of the gum, and rapidly decay and fall to pieces, so that many idiots before they are twenty years of age have nothing but a few blackened stumps. The teeth generally decay most rapidly in the upper jaw. I believe that deformities of the heart are also common with congenital idiots. The organ is small and feeble; the valves close imperfectly, or there is an open foramen ovale; the circulation is feeble, and the extremities are habitually cold. Other deformities are met with in different parts of the body; the most common are hernia, clubfoot, wad-shaped fingers, squinting and rolling of the eyes.

Congenital idiots are seldom well made, often of the scrofulous diathesis; sometimes, however, they are strong and good-looking, with well-formed heads, good teeth, and no deformities whatever.

Congenital idiots present every variety of mental power or feebleness, and are not less educable than other classes.

Cretinism.

While little of a scientific character has been written on sporadic idiocy, the literature of cretinism is very extensive; but it is not my intention to enter at any length into the subject, though, for the sake of clearness, it is difficult to avoid going over what seems to be the most important points made out in connexion with this interesting disease. The definition of cretinism reposes more upon its etiology than upon its pathology; for, in our imperfect state of knowledge, etiology sometimes guides us to true pathology, and pathology leads us back to new views on etiology; as difference in the cause implies difference in the effect, and a different effect implies a new cause or combination of causes. Cretinism is an endemic disease, its areas being generally capable of clear definition. It is very common in certain valleys of the Alps, where it has attracted most attention; but it is not confined to any quarter of the globe. It has been found amongst the Cordilleras as well as the Himalayas, the Pyrenees as well as the Crapak Mountains, in Chinese Tartary, in Sumatra and Java, in the Isthmus of Darien, in Madagascar, and many other places. It is not confined to valleys of mountainous countries, being found in parts of the Terai, at the foot of the Himalayas, in some villages in the Punjab, and in the plains of Lombardy and the Black Forest of Baden. It is, however, most common in shut-up valleys, and has a close, though unexplained, connexion with goître. Nowhere does cretinism occur where goître is absent; but goître may occur where cretinism is unknown or rare.

The specific cause—something in the air, earth, or water, or in all of them, like the malaria of intermittent fever—is only known through its effects on the human body. It has not yet been isolated either by inference or experiment; and, indeed, we know less of it than about the character of paludal miasma. The accumulation of decaying vegetable matter in certain situations, and at known temperatures, will surely cause the appearance of ague amongst those dwelling near; but there are many shut-in valleys, resembling in all respects the usual haunts of cretinism, where, nevertheless, it is never heard of.

It seems to be more common on rocks of magnesian limestone, but is also known to be rife in valleys where the primary or schistose rocks are the main or only formations. It is not hereditary in the constitution of the parent. A man and his wife with hereditary tendency to insanity cannot avoid transmitting the vice to the children, should they flee to the ends of the earth; but parents leaving the valleys of the Aosta, or the Isère, or the Valais, for places where cretinism is unknown, leave behind them the danger of having cretin children. On the other hand, the intermarriage of goîtrous parents, or where the woman is cretinous, as sometimes happens, increases the danger of cretinism in the children.

In describing the appearance of cretins, authors have collected deformities from different cases, so as to make up the portrait of a monstrous creature, which assuredly is rarely to be met with. The most characteristic traits which occur in cretins are the stupid, monotonous facial expression, the nose depressed at its root and broad at the wings, the remarkable distance between the eyes, occupied by a hollow, from which the root of the nose seems to issue; the eyes dull and heavy, the broad zygomatic arch, the wide mouth, the broad lips, and the thick tongue. The teeth are generally bad, and soon come to decay; sometimes first teeth are not renewed. Cretins rarely attain the usual height. Many are dwarfs no higher than three feet. The limbs are often disproportioned, the walk awkward—what is called the "Bärengang" or bear gait in the German parts of Switzerland. The neck is generally short and thick, and from one-third to two-thirds of cretins are said to have goître.

Virchow has called attention to the early synostosis of the spheno-basilar bone in cretins, which causes the clivus to descend with an unusually rapid slope, so that it approaches to a right angle in relation to the body of the sphenoid. This prevents the elongation of the base of the skull, and consequently of the brain, from the foramen magnum to the crista galli.

In a new-born cretin which was carefully dissected by Virchow, the whole length of the base of the brain was found fourteen to sixteen millimetres less than usual, and the angle between the posterior part of the sphenoid and basilar bone was from 42° to 46° more acute than normal. The synostosis of the sphenoid and basilar bone had already taken place. I have no intention of entering at present into the question whether this condition is a necessary cause—a frequent or invariable concomitant of cretinism. Virchow does not explain, on physiological grounds, why a diminished growth of a part of the base of the brain should cause idiocy. The sutures on the upper part of the skull generally remain open in cretins, allowing the hemispheres to increase to a normal size, and even on the floor of the cranium there are other sutures which remain open, and allow room for the growth of the base of the brain, both laterally and longitudinally.

The older observers paid more attention to the state of the brain than to that of the sutures; but they could not fail to observe the unusual steepness of the basilar portion of the occipital bone, which made it descend almost at a right angle to the sphenoid. Ackerman[1] had observed this in two skulls which he had seen in the Museum at Padua. Foderé[2] quotes the dissections of Malacarne to the same effect. A greater size of the jugular foramina in cretins has also been noticed by many observers. A collection of the lesions and deformities found in cretins is contained in the Sardinian Report on Cretinism. They are of a diversified character; and one reason of this, no doubt, is, that all cases of idiocy occurring in districts where this form of idiocy is endemic are classed as cretins: thus, hydrocephalus and cerebral hæmorrhage are put down as lesions found in the brains of cretins; but we have no proof that the miasma which causes cretinism, whatever it may be, acts so as to exclude other causes which produce idiocy in countries where cretinism is unknown, and strangers coming into the countries where the disease is endemic have children who become cretins. It has been known from time immemorial in Valais, as well as the canton of Berne and elsewhere, that mothers who pass the last months of their pregnancy and bring up their children for several years at high elevations where cretinism is unknown, can thus save them from the disease. According to the Count Rambuteau, Prefect of the Simplon in 1813, many children remain perfectly healthy for the first five or six months of their lives, frequently as long as three or four years, when suddenly symptoms of cretinism appear, and rapidly progress. The children who afterwards become cretins are often born with a small goître; but many cretins have no goître, and many children with goître are not cretins. Children with a well-marked tendency to cretinism can be often saved from it by being shifted to places where it does not appear. Sometimes the intelligence of the cretin seems to grow like that of other children; but it gradually begins to be arrested. Guggenbühl, who was the first to educate cretins in a successful manner, laid considerable stress upon conveying his patients to a healthy situation beyond the reach of the endemic cause. He claimed to have made a number of total cures, and the improvement of his cases was attested by many competent authorities, some of which are cited in his work. I have, however, been assured by the teachers of several training schools visited by me in Switzerland, that cretins do not seem to improve under training any faster than idiots of other classes.

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[1] See Ackerman über die Kretinen eine besondere Menschenabart in den Alpen. Gotha, 1790, s. 33–34.

[2] Foderé, Traité du Goître et du Cretinisme. Paris, An. VIII., para. LXXXI., p. 145.

Doctors Eulenberg and Ferd. Marfels, in a little book, Zur Pathologischen Anatomie des Cretinismus, Wetzlar, 1857, confirm the observations of Virchow by new ones of their own.

Idiocy by Deprivation.

This condition, if it be not idiocy, simulates it so closely, that it is needful to say a few words about it. As is finely stated in the aphorism quoted by Sir William Hamilton: *Cognitio omnis a mente primam originem, a sensibus exordium habet primum.*

A being deprived of sight and hearing, the two senses most useful in perception, is, even when in possession of a potential intellect of good capacity, in reality an idiot as far as its relations with the outer world go. It is a mere recipient of sensations, from which it cannot, without some very special culture, deduce sufficient explanations of the phenomena of the outer world for it to act like a reasonable being. The famous case of Laura Bridgman, as well as that of Meystre, and some others less known, are memorable examples of what a very skilful and patient education may do in awakening the dormant activity of the mind when the principal channels of sense are cut off.

Laura Bridgman lost her sight and hearing at the age of two years; the sense of smell was at the same time almost destroyed, and that of taste much blunted. She had suffered from fits in infancy, but seemingly without injury to her mental faculties. Meystre was blind and deaf. They were both taught the finger-language, and Meystre was even taught to speak a little. Idiocy by deprivation is like a seed which does not sprout because it is kept away from sunlight and moisture, while incurable idiocy is like a seed in which the germinal faculty has been destroyed; and the higher grades of idiocy resemble seeds in which the germinal capacity is much impaired, and the growth enfeebled, so that they require unusual stimulants. I have met with several cases of idiots who were deaf; with others who were blind, or nearly so; and either of these deficiencies, of course, when combined with mental torpor, is a very serious bar to instruction. In a child of ordinary capacity, deafness is a much greater obstacle to instruction than blindness; but it may admit of some doubt whether this is the case with idiots, at least with idiots of the lower grade, where the power of seizing abstract ideas is deficient, and who often are unable to learn more of the outward world than is gained from observing its more superficial phenomena.

Kaspar Hauser, who appeared suddenly on the 26th of May 1828 at the Halle gate of Nuremburg, at first resembled an idiot in many things, but rapidly improved under judicious instruction. It appeared, from the explanations which he was able afterwards to give, that he had been shut up since early childhood in a small cellar, neither seeing the light nor hearing any one speak. It turned out, however, that he had some faint recollections of a happier life, and remembered a few Hungarian words. He was murdered on the 14th of December 1833, probably by some one who wished the secret of his origin to remain undiscovered. It may be noticed that the truth of his singular story has been questioned—as far as I can judge, on insufficient grounds.

Eschricht, a Danish physiologist, brought forward the theory that Kaspar Hauser was a case of cure from idiocy—a notion easily refuted by his old preceptor Daumer.[1]

There are a number of accounts of children who have been carried away by wolves, monkeys, or other wild animals, or who were caught straying in the woods. Many of these are perhaps as little worthy of credit as the story of Romulus and Remus; others are strongly attested, and seem to be true, though the details are perhaps heightened by the desire of exciting wonder or horror. In some of these cases it appears that the suggestive education which had been given to the brutish propensities had so overpowered the intellectual faculties, that these unfortunate beings remained in a state partaking of imbecility or insanity for years after, and in some cases for life. They disliked clothes, were dirty in their habits, fed like brutes, and used few words, or could scarcely be brought to speak at all.

I have only to add at present, that in trying to reduce all cases of idiocy which I have seen to these ten classes, I do not imagine that this arrangement will be found final. The sure advance of pathology must lead to changes in nosology, and the Protean forms of disease now and then present groups of symptoms for which the mind has prepared no satisfactory class. New divisions might even be anticipated, were I not averse to the premature fabrication of classes and names. The term choreic idiocy is not unfrequently used; but though chorea is not rare as a complication with idiocy, I have seen no case where it could with truth be put down in a causal connexion.

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[1] Enthüllungen über Kaspar Hauser, von E. Fr. Daumer. Frankfurt, 1859.

ARTICLE III.—*Three Cases of Pleural Effusion Treated Successfully by Pneumatic Aspiration.* By JAMES P. BRAMWELL, M.D., L.R.C.S., Perth.

CASE I.—J. Y., shoemaker, aged 29 years, had always enjoyed good health till June last, when he was, after exposure to cold, seized with severe pain in the right side as far down as the region of the liver. The case turned out to be one of diaphragmatic pleurisy, and ended in extensive effusion. He was repeatedly blistered by his medical attendant, Dr M'Millan, of Stanley, who also administered both saline and mercurial diuretics, but there was no substantial improvement. Being called in consultation four weeks after the seizure, I found well-marked symptoms of effusion in the right pleural cavity, namely, dulness on percussion, bulging of intercostal spaces, all but entire absence of respiratory murmur, bronchophony, and œgophony. There were no febrile symptoms, but the patient looked anxious and restless, and had two attacks of distressing dyspnœa, combined with a tendency to syncope. Judging from the last-mentioned symptoms that the case had become critical, and that further delay would be very dangerous, we resolved to tap the chest next day. Accordingly, after administering some brandy and water, a trochar was pushed boldly between the sixth and seventh ribs, and the aspirator (Dieulafoy's) adjusted. The fluid flowed freely, and proved to be serum of moderate density. Seventy-three ounces in all were removed. He bore the operation well, but complained of chest uneasiness, and coughed a good deal as the process drew to a close.

Five days after, we again subjected the chest to a careful examination, and as there was still considerable dulness below the angle of the scapula, I aspirated in the eighth intercostal space, over the suspicious region, and also in front, but failed to find any more effused fluid.

There can be little doubt that the dulness and want of respiration, towards the posterior part of the pulmonary lobe, was owing to adhesion by a layer of effused lymph.

The fluid did not re-accumulate. Condition of patient five months after the operation (17th December):—In good health, well nourished, and has returned to his work as formerly; has no cough or shortness of breath. Condition of right lung:—Breath-sounds and percussion-note normal; at the old suspicious quarter, where there is still a want of complete expansive power, some degree of dulness remains.

Here we have reason to assume that all diseased action has long since ceased, but that there are permanent adhesions at the lower lobe of the right lung, which, although compromising its function in some small measure, are not calculated in any serious manner to interfere with the health or usefulness of the patient.

Case II.—Mrs M., aged 30. Was first called to see her after a miscarriage, in which she had lost a good deal of blood. I removed the placenta, and the hæmorrhage ceased. I found her suffering from organic disease of the heart, mitral incompetency, with some degree of compensatory hypertrophy; also was informed that she had had more than one attack of rheumatic fever. Ordered her a good stimulating diet, and 15 drops of the muriated iron tincture twice daily.

Ten days after, the weather became cold and damp, causing a return of her rheumatic symptoms. For this she was ordered the ordinary treatment—alkaline diuretics, with a morphia pill at bedtime. Urine very scanty, and loaded with lithates. Began to complain of pain in the right side and a degree of dyspnœa some days afterwards. Lungs much congested, sputum bloody. To take an infusion of digitalis, with acetate and carbonate of potash combined. No diuresis. Dyspnœa much increased; indeed there is almost constant orthopnœa; face very anxious, pulse feeble and irregular, feet and legs slightly œdematous.

There are now unequivocal indications of pleural effusion on the right side, which is dull on percussion, and measures more than the opposite one by two inches; bronchophony and œgophony are both audible, and also fine crepitation at the base of the lung. A No. 1 trocar and canula were thrust into the eighth intercostal space, and about 30 ounces of sanguineous serum removed by the aspirator. The patient complained little during the operation. To continue diuretics as before. On the fifth day the orthopnœa was as urgent as ever, and a stethoscopic examination indicated clearly that a fresh effusion had taken place.

I repeated the aspiration in same region, and removed about 45 ounces of serous fluid, without any admixture of blood, and which on cooling coagulated. Much relief followed this operation, both as regards her breathing and all her other symptoms. It is now ten days since, and there are no indications of a return of the effusion; besides this, the diuretics have begun to act freely, and her anasarca is rapidly disappearing. Can now sit up for several hours at a time. To resume the muriated iron drops and stop all other medicines. Two months have elapsed without any return of the effusion.

It is somewhat difficult to determine whether this was a case of genuine pleurisy or one of hydrothorax, from the additional strain of a rheumatic attack upon a heart previously extensively diseased. But be this as it may, there can be no doubt as to the benefit conferred on this poor sufferer by pneumatic aspiration, and the averting of an abrupt summing up of a life which, although a precarious one, may yet hold out for some time to come.

Case III.—While in the Highlands in August last I was requested to examine a deerstalker, who, after a severe chill, had been seized with stitch in the left side and dyspnœa. His pulse was feeble and very irregular, so much so as to make me suspect some organic cardiac lesion; but the history of the case, and a thorough examination of the chest, dissipated this illusion. He was a powerful young man, capable of, and often subjected to, severe exertion, and had enjoyed perfect health until his present illness.

Condition of Chest.—Heart displaced towards right side, apex beating on a line with the sternum, percussion-sound on left side absolutely dull all over, the natural respiratory murmur was all but *nil*, and there was both bronchophony and œgophony. The circumference of the left side was 1½ inches more than that of the opposite one. Respiration and percussion on the right side normal, the former somewhat puerile. Here we have a full-drawn picture of acute pleurisy, followed by extensive effusion. Believing this patient to be in imminent danger of death from syncope, the heart's action being so feeble and irregular, I telegraphed at once for Dieulafoy's aspirator. Next day, after administering to the patient a half-glass of brandy and water, I pushed No. 3 trochar and canula into the seventh intercostal space, on a line with and two or three inches behind the nipple. The fluid, which happily proved to be pure serum, was reached at once. The aspirator was now attached, and about 50 ounces of a coagulable fluid, of high specific gravity, evacuated. Towards the end of the operation, however, there was much troublesome cough, with distressing dyspnœa, which caused me to desist sooner than I could have wished, as the fluid was still flowing freely. There were no bad symptoms after the operation; and next day the heart had returned to its normal position, the pulse was steady, the respiratory murmur audible as low as the sixth rib, and the percussion-dulness limited to the regions below this point. It was quite clear that here we had no adhesions to contend with, and that the lung, in twenty-four hours' time, had regained two-thirds of its normal expanding power. I watched carefully for several days to see whether nature would dispose of the remaining fluid, or if a fresh accumulation would take place. Fortunately there was a gradual lessening of the dull region, with a simultaneous extension of the respiratory murmur. In the circumstances no diuretics were thought necessary, but a tonic of quinine and iron was administered, and the lower part of the side painted with the tincture of iodine. It is now four months since the operation. During this time I have twice heard from my patient, who has lost all his chest symptoms, and is in good health, although his strength is not yet quite equal to what it was before his severe illness.

Remarks.—In a Paris dissecting-room we once saw the body of a powerful man, plump and muscular to a degree; one of his sides was more prominent than the other; it was marked all over by recent vesication, and to percussion was dull as a board. Death had been caused by an extensive pleural effusion, the treatment being apparently of an entirely medical character. It is much to be feared that this melancholy *dénouement* has been but too often repeated since then.

This general reluctance to draw off a pleural effusion has been based on two fallacies. First, that medical means are in general quite equal to the disposal of the intrathoracic effusion; and, second, that paracentesis thoracis is a very dangerous operation. So far, however, from chest effusions of any magnitude being removed by medical means alone, there is a growing conviction that the opposite is the case, and that the majority of such cases will resist the power of all medicaments combined. Be the cause what it may, a largely distended pleural cavity disposes of its fluid contents very slowly, if at all. Patients in this condition are liable to many dangers, immediate and remote, and often die in a very sudden manner; a twisted aorta or plugged pulmonary artery showing but too plainly what pent-up fluid can do. Besides all this, it is well known now how often a pulmonary phthisis grows out of a neglected empyema treated on the expectant system. There is also another danger, even although the fluid is removed by slow absorption; the lung is often rendered more or less useless by permanent adhesions. We have seen a whole lung compressed into the size of a small orange, and rendered utterly functionless by this cause alone. As to the second fallacy—namely, the great danger of paracentesis thoracis—there can be little doubt that this has been greatly exaggerated. As formerly performed by a large trocar and canula alone, the benefits of the operation were more restricted, and its dangers increased, but even this was surely preferable to death from sudden asphyxia. We are greatly indebted to the noble Trousseau for the heroic way in which he pled for the performance of this operation, and laboured to lessen its dangers by his valve of goldbeaters' skin; and now his distinguished pupil, Dr Dieulafoy, by calling in the aid of the aspirator, has perfected the procedure.

It would be unjust, however, not to mention what we owe to Dr Bowditch as having long used the aspirator on the other side of the Atlantic;[1] as also the praiseworthy efforts of Dr Allbutt of Leeds, to rescue from oblivion paracentesis thoracis, by showing the great merits of tapping in both pleural and pericardial effusions.

There is sufficient evidence now to prove that the pneumatic aspiration of intrathoracic effusions is an almost harmless operation. To subject our patients then to blistering and mercurialization, with all its attendant evils, with a simple appliance such as this at hand, is simply barbarous. Let the operation be early performed, and do not let us allow the continuance of the symptomatic fever to prevent us, for it has been proved that the removal of fluid, even in such circumstances, can be achieved in some cases at least by one tapping, and that, with a rapid diminution of the general pyrexia.[2]

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[1] See Dr Bowditch's letter to Dr Allbutt, "Practitioner," April 1873.

[2] See Dr Sidney Ringer's cases of pleuritic effusion in the "Practitioner" for December 1873. (Also a most valuable paper by Dr Warburton Begbie in this Journal, June 1866.—Ed. *E. M. J.*)

There can be little doubt that with such facts before us, paracentesis thoracis by pneumatic aspiration will become a very common operation, and if so, we shall hear less frequently of deaths from pleurisy. For, how, we ask, can this affection, if uncomplicated, kill at all, unless by the effects of effused fluid?

ARTICLE IV.—*The Superior Value of Artificial Premature Labour and Turning, over the High Forceps Operation and Craniotomy, in Cases of Contracted Pelvis; with Cases.* By ALEXANDER MILNE, M.D., Corresponding Member of the Gynæcological Society of Boston, etc.

(*Read before the Obstetrical Society of Edinburgh,* 12*th November* 1873.)

THE question of the best line of practice (that is to say, the safest for both mother and child, but especially the former), in cases where the pelvis is abnormally small—whether at the brim alone or embracing all diameters—is one of the most important in practical midwifery. It has been ably discussed from time to time, and by none, I believe, more ably or exhaustively than the late Sir James Simpson, whose views will be found at great length in the first volume of his obstetric works. These views, in passing, I would strongly commend for perusal and mature consideration by any who may have been struck, staggered, or *biased* even, by recent German statistics.

Having myself also discussed this subject before our Society, in papers with cases, some seven years ago, I should not, perhaps, have thought of resuming the topic at this time, but for the fact that at our last meeting old embers were disturbed, and views promulgated which I believe to be of no little hurtful practical tendency. These views I shall examine shortly; but before doing so I feel called upon to make a remark or two on some assertions which were made at said meeting, during the discussion, by certain Fellows, particularly as it is relevant, and conducive to the elucidation of this important subject.

It was broadly, and apparently with earnestness, affirmed that we had very few, if any, extremely contracted pelves; that, *en verité*, they were almost all imaginary—the invention of men panting for renown, if not thirsting for immortality. Now, as regards such an assertion, I must observe that I believed it to be thoroughly misrepresentative of the real facts. Were it not that such cases were well known to exist, I might ask, Why should we *not* have cases of pelvic contraction in Edinburgh? What are the causes of pelvic deformity? Why, soft bones and sedentary pursuits! Well, have we no rickets here, and no sedentary women? Ay, plenty of these, and therefore in abundance the means of producing contraction. I grant, however, that we may have fewer cases proportionally than are to be found in more densely packed manufacturing cities, and more especially in the vast world of London. Yet instances of that degree of limitation of the antero-posterior diameter of the brim, especially preventing a fully-developed cranium from descending in obedience to the natural efforts and powers, are tolerably numerous amongst us.

Secondly, and before coming to the marrow of the controversy, let me refer to another statement made during the discussion on Dr Macdonald's paper, more particularly as it forms a fit preliminary to the proper contrasting of the different operations or alternatives in cases of pelvic contraction. It was actually said, with all due seriousness, that correct pelvimetry was an impossible thing—a kind of delusion and snare. In reply to this strange doctrine, I beg to affirm that it is no such thing; and, moreover, that the practitioner who, in these days of anæsthetics, cannot form a pretty correct estimate of the size of a given pelvis, is, to say the least, very much at fault. Where, as a rule, is the difficulty of inserting the hand into the pelvis and up to and above its brim? of gauging all its different diameters, of leisurely and carefully measuring them? Why, nowadays, surgeons are inserting their hand into the rectum, up to the sigmoid flexure even! Then there cannot surely be any Fellows who do not know the breadth of their own hand. If so, in presence of such ignorance there can be no skilled obstetric practice. Cases were cited where craniotomy had been performed, say in a first labour, and where the child came facilely and swiftly home on future occasions; and no little stress and emphasis were laid on such. The practitioner, too, in some of these, it was said, had strongly recommended the women to submit in future to the induction of premature labour. Well, no doubt this has happened over and over again, and no doubt it should *not* have happened over and over again. No obstetrician is warranted in proposing, far less in urging, artificial premature labour on the simple ground that perforation was once found necessary. No; there are causes sufficient connected with the uterus itself, as well as the fœtus, to justify it even where the pelvis is ample. Only a short time ago I had to perforate in a case I saw with Dr Pridie, where the fault and occasion were the abnormal enlargement of the fœtal head. Both Dr Pridie and myself readily made out that the pelvis was roomy enough, that the fault was fœtal, not maternal; and we would not in such a case have even dreamed of proposing artificial premature labour for the future. It is just one of those cases where the child will, in all likelihood, in future glide easily, if not speedily, home. When one meets with a case, then, demanding craniotomy or cephalotripsy, one should examine and measure the pelvis carefully and thoroughly; and if there should happen to be any doubt regarding the diameter, let the dubiety be confirmed or dispelled, if need be, by higher skill; for it is a paramount duty to abolish uncertainty in such grave cases. I am well aware that the induction of premature labour has been frequently recommended where there has been no attempt even at any kind of pelvimetry, and where the only ground for such a proposal was the abnormal size of the fœtal head. This is a most reprehensible thing, and fitted to bring discredit on what I shall emphatically term a grand conservative operation. We *must* have pelvimetry in cases where craniotomy has been necessitated; and with chloroform there is no excuse for this measurement being slipshod or inefficient, or for men recklessly undertaking operations, say, if you will, with the view—the wretched view—of achieving renown. But a truce to these preliminary observations; and now let me come to the pith of the question discussed by Dr Macdonald in the paper before referred to.

The gist of that paper was, without exaggeration, as follows:—In contracted pelvis try delay first, and if that does not succeed, try the forceps. If this instrument fails, resort to craniotomy. Further, the operation of inducing premature labour, in cases of contracted pelvis, was to be discouraged, nay, abandoned. I confess, when I heard these views promulgated before the Edinburgh Obstetrical Society, I could hardly credit my own ears, so startling seemed the doctrines; nay, more, so sacrificial and destructive seemed the creed. The spirit of modern British midwifery was certainly ignored, and what might be the occasion and cause of it? Why, not personal experience, for the Doctor candidly acknowledged that this was limited rather, but certain recent German statistics. Well, German statisticians possess a reputation for carefulness, but I object to settling any practical obstetric question by a heterogeneous mass of statistics, gleaned from the most diverse quarters, and embracing, often, alongside of expert work, the miserable results of, I shall not say bungling, but inexperienced manipulation. I should no more think of altering my views (and practice) as regards this question on such grounds, than I would of impugning the propriety and challenging the safety, in general, of the operation of ovariotomy, because some eminent brother in *Deutschland*, perhaps long immured amidst musty gynæcological pamphlets, happened to glean a number of failures from the records of his *Vaterland* and elsewhere. The numerical method, to be of any practical service, must be a homogeneous thing, and include only the practice of experts; moreover, as and while practice differs widely in different lands, let not the statistics of one country rule or shape, necessarily, the practice of another.

But now I must proceed to show that the operations of artificial premature labour and turning, in cases of contracted pelvis, are superior to craniotomy and the long forceps, not to speak of protracted delay and trusting to nature. That is to say, in short, that by the former means more maternal and fœtal lives are preserved, or ought to be. First, as regards artificial premature labour. Here, with reference to the mother—and I speak of well-authenticated British practice—the safety of the operation is a settled matter.

There is little dubiety about it; it is almost as certain as the diurnal revolution of the earth. I might almost go the length of saying that, if a single woman is killed by it, the fault must attach to the operator, not to the operation, or perhaps to some stupid operation having been performed. Spiegelberg's 18 per cent., to me, represents simply so much deplorable and condemnable bungling. I could have cited numerous British names, of pre-eminent standing, by way of endorsement—Hamilton, Simpson, Ramsbotham, etc., etc.—but there is little need. For myself, permit me to say that I have done the operation often, and always, as regards the mother, without evil, far less fatal issue. No more, then, need be said under this head.

Now, let us inquire into the question of fœtal safety. Here again I confine myself to good British practice, and I find something very different from Spiegelberg's mortality of 28 per cent. For example, the late Professor Hamilton saved 42 out of 46 children. The late Dr Ramsbotham saved 49 out of 91. This was a much smaller percentage than in Dr Hamilton's practice, but the explanation is transparent. In the first place, many of his women had gone too far, necessitating reducing instruments even; and, secondly, ergot was given in 34 cases—very hurtful practice indeed; that is to say, in premature delivery. No good accoucheur, nowadays, would think of inducing premature labour by means of this medicine in such cases.

I have induced premature labour now in contracted pelves about fifty times. I regret to say that, from want of time chiefly, I am only able to give an analysis of 38 cases. These embrace only six women. Five of these have contracted conjugate only, while the sixth has all the diameters lessened—an example of the *pelvis æquabiliter justo minor.* Of the five women whose pelvic brim is contracted antero-posteriorly, four measure about 3 inches, while the fifth has a conjugate of only 2¼ inches. (Of the correctness of this latter I am highly assured, because the late Sir James Simpson confirmed my measurement, he having delivered the woman by cephalotripsy, along with Dr Black, several years ago.) The sixth case—the one whose diameters are all lessened—has a conjugate of about 3 inches. Now I shall begin with the worst first, and briefly detail her obstetric history. This is Mrs S., with a conjugate of only 2¼ inches. In this case also the outlet is somewhat narrowed, but, singularly, the other parts of the pelvis are not much so. She has been delivered six times. Her first was cephalotripsy, by the late Sir James Sinpson, aided by myself. It was uncommonly difficult, after perforation and crushing, to get the child (a female) home. It occupied, I think, about twenty minutes, Sir James becoming exhausted during the traction. Here it was agreed by us that the proper line of treatment in future, if the shadow of a chance was to be given to the child, lay in the induction of premature labour. This decision I communicated to the woman, and I am bound to say that, so far from her having been averse to it, she evinced the greatest willingnesss, nay, anxiety, to have the natural labour anticipated. The subsequent five labours have all been under my care, and I now give a brief record of them. Her second confinement, which I induced about the seventh month, gave a living girl, which survives. Her third (delivered, so far as I could make out, about the seventh month) lived only thirty-six hours. This was also a girl. The next two, females, were dead, great difficulty being experienced in getting the head through the brim; and the last, a male, was delivered with considerable difficulty and without respiration, yet, after some exertion, was restored, and survives. These two children—the only possible ones out of the lot, for it was a pelvis necessitating perforation at the full term—I view as decided triumphs, and no miser could prize his gold more than I do these "forlorn hopes" of the *quondam* rickety mother.

As regards the other women, the one with the pelvis contracted in all its diameters has been delivered eight times. During her first two labours she was delivered by the long forceps, or high operation, both children, a male and a female, being extracted dead after (as I was informed, for she was not then under my care) a pull of about half an hour. During the last six pregnancies I have induced labour at the seventh month, all the children—two males and four females—having survived. I shall not be so bigoted as to say that a further attempt with the forceps might not have given a living child, yet seeing that the first two were extracted dead after much difficulty, I am strongly convinced of the greater propriety and safety of the plan I adopted. Let it be noted, that this was a pelvis contracted in all its diameters considerably; and for this reason, that in the paper of Dr Macdonald it was said, on the authority of Borinski and others, that such pelves were "exceedingly ill-suited for delivery by version." If such were the case, could I have saved the whole by turning? I trow not. The fact is, more children, as a rule, are sacrificed by turning in a pelvis where the contraction is confined to the conjugate, than where all the diameters are lessened; and for this plain reason, that in the latter case the diminution is rarely so great. That is to say, you may have the different diameters reduced from the normal only by a half or three-fourths of an inch or so; whereas, in conjugal contraction, if I may so speak, you will not seldom have a reduction of one to one and a half or two inches.

The remaining four women had conjugates reduced to about three inches. Mrs A. has been delivered eight times. Her first, a female, was extracted dead after twelve hours of a second stage, the long forceps being required; and her second, a male, delivered by cephalotripsy, after a cautious and skilled, yet fruitless, attempt with the forceps. The remaining six labours were by the induction of premature labour about the seventh month, and all survived, viz., five females and one male child.

Mrs L. has had nine children. The first, a female, was brought home dead after a long pull with the long forceps; the second, a female, was brought home alive by the same instrument, after much toil, and a bad recovery on the part of the mother. The subsequent seven I delivered about the eighth month by version, saving the whole, viz., five girls and two boys.

Mrs M'M. had three children extracted dead by the forceps, and one by craniotomy. I was not then her accoucheur. She came under my care during her fifth pregnancy, and on hearing her obstetric history, I resolved to induce labour prematurely. To this she most willingly consented, and I have delivered her, during a period of nine years, of six children—two females and four males—all of whom are alive.

The last case of which I have been able to preserve notes is Mrs M'A., from the neighbourhood of Kelso. She came under me a number of years ago, while in labour with her first child, and this a male. I delivered by the forceps, high operation, after half an hour's exertion, dead. Having of course measured the pelvis, I resolved that, should she again come under my care, premature labour would have to be induced and version resorted to. On explaining this to her, she readily consented. Almost every year since (for she is a powerful woman, and does not suckle her offspring) she has been under my care, and by premature induction and version I have saved them all but one, viz., four male and four female children.

These, Gentlemen, constitute the entirety of the cases of which I have preserved notes, and I must take leave to say, that *I for one* view them with no little pleasure and pride. Without egotism, I may say they are trophies fitted alike to gratify one, and to encourage and stimulate in the future.

Now, it may be said, granted that you extracted from these contracted pelves thirty-five of your thirty-eight alive, how long did they survive? how many of them are dead? how many still alive? The remark is thoroughly pertinent, for the conviction obtains in respectable quarters that these premature children are rather apt to be short-lived. It has been said, for instance, by such authorities as Virchow, that when such children succumb, say during dentition or measles, or perish ostensibly of meningitis, etc., that the real cause of death must be ascribed to the defects of prematurity—to congenital feebleness, in short. Now, I have seen nothing to warrant me viewing prematurity—that is, at and after the seventh month—as necessarily convertible with debility. I have rather observed that, when such children were carried off by disease, it was chiefly owing to transmitted defects, that is, to parental, ancestral, or hereditary taint—in short, to such ailments as characterize equally the mature and the premature. But to resume. As regards the children delivered prematurely by the method I advocate, I am able to say that seventeen of them are certainly alive still, that seven are assuredly dead; and as regards the other eleven, they have got beyond my ken, though possibly they may all be alive. In reference to the children known to be dead, let me state that the majority of them were nursed by strangers; and before the members of this Society I need not expatiate on the adverse character, as a rule, of hireling nursing. Most commonly it is not only the bottle, but a very shabby and insufficient bottle, that replaces the natural teat.

We have here, then, Gentlemen, 174 operations, with the result of saving about 80 per cent. of the children; and as regards maternal fatality, that must be set down as almost *nil.* I believe, further, as regards the children, that if I could have collected cases from the practice of the more highly skilled obstetricians of the present day, these would have shown us a much higher percentage of gain still. For be it observed that the common plan of induction thirty years ago was by ergot, or puncturing of the membranes—both unequivocally reprehensible, as surrounding the fœtus with unnecessary and uncalled-for peril.

Having shown, then, necessarily briefly, the harmless character of artificial premature labour in contracted pelvis—that is to say, where the operation has been undertaken for that defect alone, and in the absence of maternal disease—and further, that it is very conservative of fœtal life, I now proceed to consider the other alternatives, beginning with craniotomy. Here, again, brevity is demanded. This is an operation which, I presume, most of us think should not be performed but with the gravest consideration and deliberation. I have performed it frequently, I believe never unnecessarily; but I confess I have a waxing sense of the reponsibility attaching to the operator. At our last gathering the thing seemed to me to be treated rather too lightly. We were told it would be much the better way just to squash the youngsters, than to fetch them home to be probable idiots—a burden to themselves, to society, and friends. Now, I do not think that any of us are justified in using the perforator with all the unconcern that might characterize a wild and reckless knight-errant running atilt against imaginary foes. I rather think that every possible resource should be exhausted before plunging the steel. No doubt, there is a preponderance of opinion in this country in favour of the higher value of the mother's life (although, on the other hand, many able men feel unable to assume the burden of fœtal destruction), and that we are justified in saving her in preference to the child. But then my contention is, that the mother may be protected and saved without the necessity of destroying the child, and that by the operation of artificial premature labour.

But now, as regards the statistics of the operation. The books say that it kills one in five or six mothers. This cannot represent anything like expert modern practice; and suppose we say that it destroys one mother in twenty. Well, in 100 cases, that would give us five deaths. How badly does this contrast with the operation of artificial premature labour! Now, in reference to the child, it is all loss—all sacrifice, of course; you grind the infant for the sake of its mother. "Thou shalt not kill" is met with the stern decree "Thou shalt not live," and *in* goes the glistening steel. Let me not, however, moralize; only see again what inferior results we have to those furnished by the operation we praise.

The next alternative I have to notice is the forceps; and here a word or two of explanation is requisite. By the forceps here I do not allude to the beautifully conservative short forceps—the low operation. Very far from it. I have lived to experience and extol its excellences, and I trust and believe I shall never have to decry these. It is the long forceps I mean, or, better still, the high operation; that is to say, the head is lying, or dodging, it may be, above, or at the brim, and fails, owing to pelvic contraction, to descend. Nay, it will not come down without great force in such cases, necessitating great compression of the cranium. This fact alone damns the high operation, for the forceps ought not to be a compressor, but only a tractor. When you employ it to squeeze and crush the head, you prostitute it entirely. It is true you may, with a considerable fight, extract a living child, although the reverse often happens; but what of its future history, not to speak of maternal injury, to which we shall presently refer? As regards the long-forceps children—I mean their history—there is not a large mass of statistics to go upon, so far as I am aware; but not a few practitioners have noticed them merging into idiocy, the compression being great antero-posteriorly. I have noticed several such cases. Then the maternal risks must be patent to all. You have, for instance, in these high cases, to insert the forceps within the os uteri; and although (having done that myself without injury frequently) I would not be found exaggerating the risks of such a proceeding, there appears to be a widespread conviction of its dangers among experienced men. Meigs, for example, says in his work (p. 555), "A man shall hardly be justified who inserts his forceps within the os uteri. He must wait until the circle has risen above the parietal protuberance, and can no longer be felt. . . . We must not use the forceps on account simply of preternatural resistance," etc., etc. I need hardly dilate on the risks of the high operation—that is, the maternal risks—before the members of this Society. They know that laceration of the uterus is not uncommon; that inflammation and sloughing are also not uncommon. In brief, it is one of such gravity as warrants one to adopt instead a less hurtful expedient, and that, I believe, is found in the operation whose merits I seek to proclaim. It will be observed that I have not condescended on statistics under this head; and the reason is just this, that there are no trustworthy statistics to be had. There *are* forceps statistics, no doubt; but then long and short, high and low operations are jumbled together (in confusion worse confounded)—a most egregious blunder. Why, there is very little in common with them. The short forceps (the low operation) merely aids, or sub-

stitutes the natural *vis a tergo;* while, on the other hand, the long forceps, or high operation, as before seen, implies both risks to mother and child. To have given tolerably reliable statistics, then, the two operations should clearly have been segregated and kept apart. As it is, I shall not pretend to state even an approximation of the dangers; only let me repeat, that, while the short forceps is eminently conservative, both as regards mother and offspring, the long forceps, or high operation, is attended with no little danger to both.

I must now say a word or two on another alternative, or resource, in cases of contracted pelvis; and let me remark, that I should not have dreamed of alluding to it, but for the fact that it was advocated by Fellows at our last meeting. The alternative I refer to is not the cæsarean section, which would be somewhat reasonable, but just delay, and trusting to nature. Now, this is a practice that I thought had been thoroughly exploded, except in some outlandish districts not yet permeated by the spirit of modern midwifery. Were any here yet inclined to try the plan of delay, I would exhort them to look at the invaluable statistics of Collins, and to study the tables educed from these by the late Sir James Simpson. These clearly show that "the mortality attendant upon labour increases in a ratio progressive with the increased length of the labour." *I* for one, Gentlemen, shall not retrograde to the barbarous practice of keeping women in torture for three days, exhausting their strength, thinning by pressure—nay, rupturing—their uterus, reducing their vagina to a pulp, and paving the way for a hole through to the bladder, which, however more readily curable than of old, is still a disorder to be avoided, as far as lies in our power. In contracted and distorted pelves, more particularly and emphatically is prolonged delay eminently culpable, for a projecting sacral promontory, or a sharp ridge of bone, are prone to lead to rupture. But enough.

Finally, now let me again urge the necessity, in well made out cases of pelvic contraction, of inducing premature labour in preference to craniotomy or the long forceps. The forceps (long, of course, I mean) usually, if not of necessity, damaging both to mother and child, but especially the latter; and craniotomy fatal of course to the latter, and often hurtful to the former. Fitted, also, when unjustifiably or recklessly undertaken, to haunt one both when asleep and awake.

ARTICLE V.—*Case of Cephalotripsy after Turning.* By PETER BROTHERSTON, F.R.C.S.E., Alloa.

(*Read before the Obstetrical Society of Edinburgh,* 26*th November* 1873.)

IN the following very short notes of a recent case of crushing and extraction by the cephalotribe of the head of a child left in the uterus, I have nothing to state in the way of experience which has not already fallen under observation. From this case, however, those engaged in midwifery practice, more especially in the country, may find their confidence strengthened in an instrument which has afforded a safe and easy assistance in cases of fœtal decapitation, or any case of difficult delivery requiring the head to be crushed, when other means of remedy might involve a much more tedious and dangerous operation.

Dr Keiller has related, in the Transactions of the Edinburgh Obstetrical Society, Session 1868–69, a similar case to that of my own, which saves me recapitulating what he has written.

On the 16th of June last, I was called suddenly to assist a friend with a case in which the child's head had become detached from the body during efforts at extraction, and had been left in the uterus. With such instruments as were in use previous to the cephalotribe, the extraction of the head would have involved a tedious process, aggravating the mother's sufferings, running a risk of wounding delicate parts, and incurring delay, which is always more or less dangerous in such cases.

I fortunately had previously seen the cephalotribe in the late Sir James Simpson's hands, and was sufficiently struck with the useful appearance of the instrument, to purchase one at Mr Gardner's, South Bridge, Edinburgh.

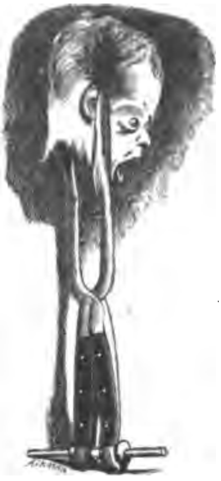

Fig. 1.

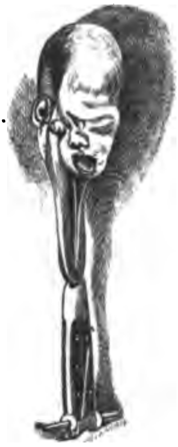

Fig. 2.

The patient I found rather depressed and anxious, and in no condition for undergoing a lengthened and difficult operation. The contraction of the uterus had ceased, and the uterus, with the enclosed head, had receded into the cavity of the abdomen.

Before proceeding, it was thought expedient to administer chloroform. Carefully inserting one blade of the instrument into the uterus, and then the other, I locked the blades, and proceeded to tighten the compressing screw. On withdrawing the instrument it enclosed the placenta, having slipped from its hold of the head. My friend then placed his hand on the uterus, and exerted sufficient pressure to throw the head more into the axis of the pelvis, and at the same time seized hold of the lower jaw to steady the head. Once more the blades of the instrument were inserted, locked, and the screw tightened. On this trial the head was firmly caught over the ears, and without any difficulty crushed and withdrawn from the uterus.

The antero-posterior diameter of the pelvis was much contracted, scarcely admitting four fingers. I learned that the woman had previously been delivered of a small premature dead child. She afterwards made an excellent recovery, and was moving about in her usual health in ten days.

Having obtained a photograph of the head in the grasp of the cephalotribe, the manner of the action of the instrument is discernible enough. The pressure exerted is sufficient to crush the cranium and face into a frightfully distorted form; but that is of little consequence when so successful an operation can be so easily achieved.

ARTICLE VI.—*The Sewage Question; with Remarks and Experiments showing the Inefficiency of our Present System of Trapping.* By ANDREW FERGUS, M.D., Glasgow.

(*The Opening Address, Session* 1873-74, *of the Sanitary and Social Economy Section of the Glasgow Philosophical Society.*)

THERE are few subjects on which the public mind is more bewildered than that of the sewage question; and to such an extent does this prevail, that some men who formerly felt interested in it have sunk into a state of apathy, and have become persuaded that there is no satisfactory solution of the matter. Perhaps this is not to be wondered at, if we consider the variety of plans suggested, and the diversity of opinions expressed on the subject. One great cause of this diversity, we think, is that the question has not been broadly looked at. It is many-sided; but writers have not recognised this, and have only looked at it in one aspect. An engineer looks at it from his own point of view, and a grand engineering scheme is the result; an enthusiastic agriculturist does the same, and sewage irrigation is to solve all our difficulties and enrich the country; the sanitarian looks to the public health, and tells us to keep all excretal matters out of the sewers, and so prevent the generation of sewer-gas; while the general public, offended by the unpleasant odours of our rivers, urgently demand their purification. Now, the question is certainly an engineering and mechanical one; it is also agricultural, economic, and chemical; but it is, above all, sanitary; and, to arrive at anything like a satisfactory solution of it, we must consider it under all these aspects, without allowing any of them unduly to influence us, or having our minds warped in any one direction. To-night we must chiefly confine our attention to the sanitary aspect of our subject.

Our first question, then, is, What is sewage? We reply, It is a mixture of rainfall (including also subsoil drainage), household slops, the excreta of man and animals, and the organic and inorganic refuse of public works. As regards the first of these,—the rainfall,—it will, of course, vary in different localities, but must in all cases be regarded as beneficial, inasmuch as it will assist in flushing the sewers. In addition to the rainfall, we have often the contents of various small streams or burns which have been converted into sewers by having been arched over. This is a most pernicious practice, because the beds of these streams are always irregular, retarding the flow of the sewage, and affording facilities for matter to accumulate and decompose in their crevices and irregularities, and thereby occasioning much evil. The entrance of subsoil water into our present system of sewers is also an evil, because, when it can do so, the contents of the sewers and the gases generated in them can also escape, polluting the soil and any water that may be drawn from it for domestic use.

Of course this evil will be primarily felt in small towns and villages, where the inhabitants depend upon wells for their domestic supply; yet, even in cities, the constant pollution of the earth by the escape of sewage must in the long-run become deleterious.

As regards the other matters in sewage, we must enter a little more into detail, by saying that, with very rare exceptions, the really injurious ingredients are the organic, or, in simpler language, those which are capable of decaying. To estimate, therefore, the amount of what is hurtful, we must inquire how much is organic in the other constituents of sewage.

As given above, let us take house slops, and we shall find, 1st, of polluting ingredients, the soap with which we wash, and the dirt from our clothes and bodies. Now, a large portion of the soap consists of soda and potash, salts which cannot under any circumstances become deleterious or polluting. 2d, We have the organic matter derived from the cooking of food and the washing of cooking utensils and dishes. Chamber slops come under the next head, viz., excreta; but I do not think it impossible to keep these separate from other house slops, as will be shown when we come to treat of remedial measures.

We have dwelt thus prominently on house slops, because we have repeatedly heard it objected to all dry systems that they do not deal with these, and they are spoken of in such terms as might lead the unlearned to believe them equally injurious with the excretal matters.

We think that the sources of organic contamination from house slops are confined to, 1st, Soap, mixed more or less with animal matter from our bodies, and the washing of our clothes; 2d, Water containing organic matter from the washing of pans, dishes, etc.; 3d, Water in which vegetables have been cooked. We need not particularize water in which meat has been cooked, as it is to be hoped it may generally be utilized for food.

If the foregoing be a correct account of the organic ingredients in house slops, it will be seen that *they* cannot be charged with any considerable amount of organic pollution, and might, as far as Glasgow is concerned, be passed with impunity into the rivers—our large domestic water-supply and great rainfall so diluting them, as to render them innoxious. The water of such rivers would not be more impure than the standard allowed by the River Pollution Commissioners, which is as under.[1]

The same cannot be said of the next ingredient that enters into the composition of sewage—viz., the excreta of man and animals. We are inclined to charge these with being, in a high degree, the cause of our present difficulties, and believe that ignorance has had much to do with converting our streams and rivers into gigantic cesspools. We use the term "ignorance" advisedly, for we are sure that when water-carriage was first introduced, the results that would flow from it were not anticipated—in fact, were never investigated. The idea acted upon was, that if these offensive matters were once out of sight, there was an end of them, and no evil consequences were ever dreaded. It was known that water, or rather the air dissolved in water, would oxidize excretal matter, but bitter experience has very sadly proved that no river in the kingdom can oxidize the excreta from the cities on its banks. Yet water-carriage has been adhered to with the most wonderful tenacity, and schemes without number have been devised to purify the water, but none of them, so far as we are aware, has ever been completely successful.

[1] Any liquid containing, *in suspension*, more than three parts by weight of dry mineral matter, or one part by weight of dry organic matter, in 100,000 parts by weight of the liquid. Any liquid containing, *in solution*, more than two parts by weight of organic carbon, or three parts by weight of organic nitrogen, in 100,000 parts by weight. Any liquid which shall exhibit by daylight a distinct colour when a stratum of it, one inch deep, is placed in a white porcelain or earthenware vessel. Any liquid which contains, in solution, in 100,000 parts by weight, more than two parts by weight of any metal except calcium, magnesium, potassium, and sodium. Any liquid which, in 100,000 parts by weight, contains, whether in solution or suspension, in chemical combination or otherwise, more than ·05 part by weight of metallic arsenic. Any liquid which, after acidification with sulphuric acid, contains, in 100,000 parts by weight, more than one part by weight of free chlorine. Any liquid which contains, in 100,000 parts by weight, more than one part by weight of sulphur, in the condition either of sulphuretted hydrogen or of a soluble sulphuret. Any liquid possessing an acidity greater than that which is produced by adding two parts by weight of real muriatic acid to 1000 parts by weight of distilled water. Any liquid possessing an alkalinity greater than that produced by adding one part by weight of dry caustic soda to 1000 parts by weight of distilled water.

The last ingredient of sewage is the organic and inorganic refuse from chemical and other works—all of which, we hold, should be kept out of the sewers; and if a law were passed enforcing this, we have little doubt but that in the majority of cases the saved material would recoup the expenses of the process.

If an Act be passed compelling all such refuse to be purified in some way or other, we shall be quite prepared for a great outcry against its hardships, and expect to be told that it will be utterly ruinous to the manufacturers.

The same thing happened at the passing of the Alkali Act, yet we believe that few will deny that the saving of the material that polluted the air has resulted in profit.

Organic matter may be rendered valuable by Mr Chapman's process; but if it will not pay to do so, it ought to be removed by downward intermittent filtration, before the water from any public work is allowed to enter the sewers.

Having thus briefly considered the composition of sewage, let us now turn our attention to the mode in which, by drainage, it is conveyed away.

The primary objects of drainage being to remove surface water and dry the subsoil, that system must be the most perfect which will both receive and freely carry off the surface water, while at the same time the joints of the system must be so formed that they will allow the gradual drainage into the sewers of the water lodged in the subsoil.

Wherever this is carried out, there is at once a marked improvement in the health of the inhabitants, as illustrated by the following from Mr Simon's Report:—In nine towns out of twenty-five where there were drainage works, there was a reduction of more than fifty per cent. in the cases of typhoid; in ten, a reduction from one-half to one-third; while in four there was either a slight decrease or an absolute increase. In these four the sewage was pumped. It was quite expected that as drainage works were carried out there would be a diminution in zymotics, but experience proves that it has also a favourable influence as regards phthisis.

If the sewers were kept only for the rainfall, no injury could happen from them to the community; but from mistaken notions, and by a bad habit, which has perhaps grown upon us insensibly, we pour all sorts of refuse into the sewers, and they again pass them on to our rivers.

If we are ever to solve the problem of the sewage question, it will be by a return to nature's own laws—namely, pass the rainfall as speedily as possible into the water-courses and rivers, and apply the excreta to the soil.

If the question only involved the purification of rivers, I would not interfere with it; but I believe our present system so highly injurious to public health that I am most anxious to bring it prominently before you to-night.

If the urine and fæces are kept separate, decomposition is retarded, but it is facilitated by their admixture, and still more so by the addition of water. I hope the advocates of our present system will keep this in mind, as they often dwell on the importance of at once getting quit of refuse matter. If we did so, there would be no sewer-gas, and no need of sewer ventilation.

Fresh excreta, except perhaps as regards one disease, are not injurious to health, but become so as soon as they decompose and give off gas.

1st. By vitiating the air in general, and depriving it of oxygen gas, as all decomposition of organic matter must of necessity do.

2d. Excreta from diseased patients produce like diseases in persons previously healthy, and this in some diseases is intensified enormously by the decomposition of the excreta.

3d. Sewer air contains a putrid organic vapour, which is highly injurious to health.

4th. Decomposition of excreta is very much facilitated by their being shut up in limited spaces, with a moderate supply of air and water; therefore, our modern house and service sewers and cesspools are ingeniously-contrived chemical laboratories for the production of sewer-gas.

5th. When sewage matter in a state of decomposition is exposed freely to the air, it ceases to be injurious to health.

I now proceed to make a few remarks on some of the above heads.

1st. All decomposition is simply a burning up or combustion of the oxygen of the air with the organic matter; therefore where this goes on there must be a diminution of the vital constituent of the atmosphere, and Dr Angus Smith, in his excellent work, has shown the diminution of oxygen in the air of towns.

Possibly further investigation will show how much of this is owing to the breathing of the population, and how much to the decomposition of organic matter in middens and sewers.

Another phenomenon attending decomposition is the evolution of heat. Sewage is generally hotter than the atmosphere, and it is yet to be decided how much of this is caused by hot water from houses and works passing into the sewers, and how much is owing to the decomposition. I incline to believe that the latter plays an important part in it.

The excreta of even healthy people are injurious when decomposed, inasmuch as they diminish the amount of oxygen in the air, and we find that breathing air with a diminished percentage of oxygen has a lowering effect on the general health.

My second head is of much greater importance, and must be dwelt upon at some length.

No one who has paid the least attention to sanitary matters will deny that excretal pollution is a powerful agent in the promotion of preventible disease; but I do not think that sufficient attention has been paid to the fact that in some diseases which are generally ascribed to this source there has been a very large increase since the introduction of water-closets.

In order to show this, I have here a table, for the working out of the percentage of which I am indebted to Mr Scott—the mention of whose name at this Section will be a sufficient guarantee for accuracy.

TABLE *showing the Number of Deaths per annum in England and Wales, from certain Zymotic Diseases separately, and from other Causes taken together, for each Million of Living Population*—1838-71.

		Deaths per annum for each Million of Living Population.									
Year.	Population.	Diphtheria.	Dysentery, Diarrhœa, and Cholera.	Scarlatina.	Fevers.	Measles.	Hooping-cough.	Smallpox.	Total from these Causes.	Total from other Causes.	Total from all Causes.
1838	15,287,699	...	225	380	1228	426	596	1064	3919	18,502	22,421
1839	15,514,255	...	225	665	1010	705	526	589	3720	18,130	21,850
1840	15,730,813	...	305	1260	1092	593	390	663	4303	18,562	22,865
1841	15,929,492	...	264	889	932	433	508	400	3426	18,160	21,586
1842	16,130,326	...	472	794	1004	542	502	168	3482	18,186	21,668
*											
1847	17,150,018	...	911	857	1807	507	540	246	4868	19,814	24,682
1848	17,356,882	...	899	1181	1270	396	395	398	4539	18,497	23,036
1849	17,564,656	...	†4222	746	1045	311	547	265	7136	17,962	25,098
1850	17,773,324	...	810	752	865	398	437	263	3525	17,236	20,761
1851	17,982,849	2	1004	756	997	521	440	389	4109	17,879	21,988
1852	18,193,206	4	1196	1034	1025	321	441	402	4423	17,955	22,378
1853	18,404,368	2	1114	851	1008	266	609	171	4021	18,859	22,880
1854	18,616,310	11	†2261	984	1015	498	525	151	5445	18,078	23,523
1855	18,829,000	10	799	910	875	390	541	134	3659	18,950	22,609
1856	19,042,412	12	836	732	850	374	484	119	3407	17,100	20,507
1857	19,256,516	16	1248	723	988	310	526	204	4015	17,786	21,801
1858	19,471,291	248	822	1309	918	476	598	332	4703	18,390	23,093
1859	19,686,701	487	1046	1011	807	485	456	195	4487	17,903	22,390
1860	19,902,713	262	562	486	654	480	430	138	3012	18,227	21,239
1861	20,119,314	225	1044	451	767	450	612	66	3615	18,012	21,627
1862	20,352,140	241	622	729	920	482	603	80	3677	17,774	21,451
1863	20,590,356	316	816	1480	875	551	548	290	4876	18,137	23,013
1864	20,834,496	262	881	1426	965	400	411	369	4714	19,070	23,784
1865	21,085,139	197	1228	839	1092	406	410	304	4476	18,806	23,282
1866	21,342,864	141	†1529	547	989	513	739	142	4600	18,860	23,460
1867	21,608,286	120	1006	569	781	305	550	116	3447	18,354	21,801
1868	21,882,059	138	1482	1001	900	531	422	94	4568	17,396	21,964
1869	22,164,847	118	969	1247	829	465	495	71	4194	18,131	22,325
1870	22,457,366	120	1211	1449	798	336	530	116	4560	18,387	22,947
1871	22,712,266	111	1136	818	695	409	456	1018	4643	18,026	22,669
Mean of 5 years,	1838-42	...	298	798	1053	540	504	577	3770	18,308	22,078
	*										
,, ,,	1847-51	...	†1569	858	1197	427	472	312	4835	18,278	23,113
,, ,,	1852-56	8	†1241	902	955	370	520	195	4191	18,188	22,379
,, ,,	1857-61	248	944	796	827	440	524	187	3966	18,064	22,030
,, ,,	1862-66	231	†1015	1004	968	471	542	237	4468	18,530	22,998
,, ,,	1867-71	121	1161	1017	800	409	491	283	4282	18,059	22,341

* Details not published in official returns for the four years, 1843-46.

† Including an epidemic of Asiatic Cholera, general in 1849 and 1854, partial in 1866.

I have the deaths from the principal zymotics in England and Wales from 1838.

There is a gap from 1842 to 1847, but this is not our fault, the figures are not to be found in the Registrar-General's return. Instead of dealing with gross numbers, I shall draw your attention to the deaths per annum of one million of the population. I take as types of disease produced by excremental pollution, diphtheria, and the diarrhœal group, and compare them with the others.

Of the former there is not a single death recorded from 1838 till 1851, when it is credited with two per million. In a few years—viz., 1859—it amounted to 487, and since then it has fluctuated very much, but has never been under 100. The diarrhœal diseases varied before 1847 from 225 to 472 per million; it then suddenly rose to 911, and it is, in 1871, 1136. In some cholera years it was much higher.

Let us now turn to some of the zymotics which are not so clearly traceable to excremental pollution. Measles, from 1838 to 1842, varies from 426 to 705, while in the last five years it varies from 305 to 531. Hooping-cough in the first five years was from 502 to 596; in the last five years from 422 to 739.

Fevers, from 932 to 1228 in the first five years; for the last five, from 695 to 989. Scarlet fever, during the first five years, from 380 to 1260; in the last five years, from 547 to 1449. Smallpox, for the first five years, varied from 1064 to 168; for the last, from 1018 to 71.

At the beginning of the period, the public were not fully protected by the compulsory vaccination. As regards fevers, it is to be regretted that they are all in one group. It would have been very interesting if, as is now the case, typhus and typhoid had been in separate groups.[1]

We may affirm that sanitary science had no existence during the earlier years of this return, and I hope we may assume that the diminution in the percentage of deaths in all the zymotics in our table, except three, is attributable to the labours of the sanitarian.

But how are we to account for the slight increase in scarlet fever, the striking increase in the diarrhœal group, and the addition to the list of a most deadly zymotic previously almost unknown? What changes have been made in the habits of the people which could in any way lead to so sad a result, after all the legislation and patriotic efforts to ameliorate the condition of the people, and to lessen the enormous amount of preventible death? Certainly the only great change has been the mode by which we get quit of our excreta and refuse.

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[1] In addition to looking at individual years, let us examine the average of five years as given in the lower part of the table. As regards diphtheria, the average in the first five years after its appearance is 8, during the next five years 248, for the third five years 231, and for the last five years 121.

The diarrhœal group gives an average of 298 for the first five years; 1569 for the next, including the severe cholera epidemic of 1849; for the third five years, 1852 to 1856 inclusive, 1241, also with the cholera epidemic of 1854; from 1857 to 1861, 944. From 1862 to 1866, including the slighter cholera epidemic of the latter year, 1015. During the last five years, the mean has been 1017, which, contrasted with the first five years, shows us that while there has been no cholera in either period, the deaths in the diarrhœal group have increased threefold.

If any one had hinted to me ten or a dozen years ago that our present system of water-carriage would prove a source of disease, I would have scouted the idea; and even when I began the study of this great question, I had perfect faith in the system, and have only been driven from it step by step as new light gradually dawned on me, and the question was sifted to the bottom.

An Act was passed in 1847 compelling all the privies of London to drain into the sewers, and we all know how soon the river became foul. The pollution of the Thames should be studied by those who attribute the foul state of our rivers to chemical and other works. There is no great amount of works on its banks, neither was there any very marked increase of these after 1847; making it, we apprehend, very clear that the Acts of 1847 and 1851, which compelled all excretal matters to be drained into the sewers, passing of course into the rivers, must have been the cause of the very foul state of the Thames in after years.

It is impossible to trace when the same system was adopted in other parts of the country; but if excretal pollution gives rise to certain diseases, and if we find these diseases increase after a change more or less complete in our mode of dealing with these matters, it is but a fair inference that the change, no matter how apparently clean, comfortable, and luxurious, must be the cause of the increase.

The subject is so very important, that I hope you will bear with me if I go somewhat into matters of detail and proof. You observe by the table that not a single case of diphtheria is to be met with till 1851; and Mr Simon asks why this disease, which overran Europe in the sixteenth century, and since then has scarcely been spoken of, should recently have become a formidable disease in England. I believe the answer to be simply this, that we have been careless in the disposal of our excreta, and have been drinking water and breathing air contaminated by excretal matter in a state of decomposition. For details I refer to the second and eighth report of the Medical Officer of Health of the Privy Council.

Lately, I was asked to see a case of diphtheria in a house where a fatal case had already occurred, and I found sewer-gas getting into it through perforated pipes. In a still more recent case I found it gaining entrance through the sink.

The production of scarlet fever from excremental pollution is probably caused by the stools of patients suffering from that disease getting into the wells, or decomposing in the sewers, and infecting the sewer-gas. Dr Littlejohn, the Medical Officer of Health for Edinburgh, says, "If they took a range of houses where this had been done (viz., water-closets introduced), in the course of one year there were double the number of deaths from typhoid and scarlet fever; and any epidemic fever occurring in these houses was intensified, and assumed a character of malignant mortality."[1]

In the report for 1870 of the Medical Officer of Health to the Privy Council, I find a table of the places visited by the Inspectors owing to the increased death-rates.

The total number of the places so visited is 66, but as 6 of these are double entries, the number is reduced to 60. In 50 of these, the increased death-rate was traced to excremental pollution, producing typhoid fever in 49 of them, and scarlet fever in 16.

The table I exhibit fully bears out Dr Greenhow's opinion as to the increase of diarrhœa.

Typhoid fever, killing from 15,000 to 18,000 every year, is another disease arising from excremental pollution.

I think I need not quote authorities to prove this; but I may refer you to the researches of all modern investigators of this disease, and especially to the reports of Mr Simon, yearly prepared for the Privy Council. The origin of typhoid fever from excremental pollution is clearly proved in almost every volume of these valuable reports, either in the reports themselves, or in the appendices written by the various official inspectors.

Having thus, as I hope, proved that certain diseases are owing to excremental pollution, let us now turn our attention to the manner in which this pollution acts.

In many cases it does so by coming in contact with wells, and polluting the water; but this, as we all well know, cannot be the case in Glasgow, where the public wells have been shut up. There is, however, another way in which it acts most injuriously—viz., contaminating the air we breathe with the results of excremental putrefaction.

Before considering this, however, we may glance at our third head, the composition of the air in sewers.

It differs very much in different circumstances, but the gases given off by the decomposition of sewage are carbonic acid, carburetted hydrogen, sulphuretted hydrogen, and ammonium sulphide, and a putrid organic vapour, which is carbo-ammoniacal, giving rise, in all probability, to the offensive smell, and possibly also an active agent in producing disease. I do not enter upon the question of whether disease is produced by the putrid organic vapour arising from the decomposition of *all* excreta, or whether, in order to do so, there must also be the excreta from diseased patients. This has already been ably discussed, and is very interesting as a purely medical question; but time will not allow us to enter upon it to-night.

Generally the amount of oxygen is diminished, and there is always more or less of the putrid organic vapour.

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[1] Edinburgh Daily Review, 18th January 1872.

In one case, Dr A. Smith found that 62 cubic feet of air from a cesspool destroyed as much potassium permanganate as 176,000 cubic feet of pure air.

But you will inquire, "How can this gas get into our houses through well-trapped drains?"

I answer that it is not easy even to trap drains.

Some people imagine that, if they have a few inches of water in a bent tube, dignified by the name of a syphon-trap, no air can pass into their houses from the sewers, and that they can ensure safety by trapping and ventilation. At one time I thought it might be possible to keep the results of the decomposition going on in the sewers out of our houses, and have frequently recommended, as the best method of ventilation, that the sewers should be connected with the furnaces of the large public works.

This was quite a cherished idea, and I abandoned it with great reluctance; but experiment, observation, and reflection compelled me to do so.

If we were to connect the sewers with furnaces, or build ventilating shafts, and create a current by fire, no doubt we would have an up-cast draught of air; but I fear very little of it would come from the vitiated air in the smaller drains, but from the nearest rain-water pipe, or from the open gratings in the streets.

To illustrate how little effect trapping has in keeping sewer-air out of house drains, I show you these deranged lead soil-pipes, which, on a former occasion, I dwelt upon here, and brought forward proof to show that these perforations were caused by sewer-gas.[1] These house soil-pipes were all trapped before they entered the drains,[2] and yet you see as much gas got through the trap as to destroy the lead.

Of course, we all know that, if there is great tension in a sewer, either from its entering a tidal river or from any other cause, there will be a temporary failure of the trap, and a rush of sewer-air past it. Still I do not think such an occasional occurrence sufficient to account for what we see in these pipes. My impression is, that the cause must be one of pretty constant action, and that the following is the process going on without much cessation—viz., *the sewer-air is absorbed by the water on the sewer side of the trap, and discharged from the house end of it.*

I have not hastily arrived at this conclusion; it has been forced on me as the only solution of all the facts of the case. If the destruction of good, well-made lead pipes is occasioned by sewer-air, and if this takes place in well-trapped house soil-pipes, we are shut up to one of three conclusions—*1st*, That the trap must be defective; *2d*, That decomposition goes on in the trap itself to such an extent as to produce sufficient vitiated air to perforate these pipes; or, *3d*, The conclusion at which I have arrived, and have stated above—the passage of sewer air *through the water* in the trap.

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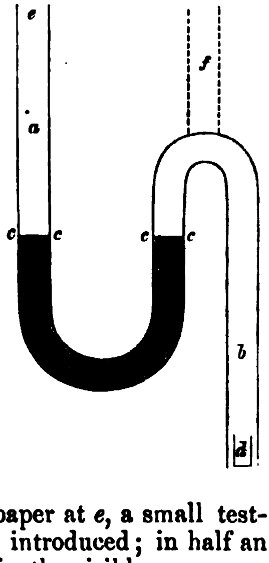
[1] At page 733 of this Journal for February 1872, there is a lithograph of these decayed pipes, and at pages 722 to 724 there is detailed account of how the lead pipes become destroyed, and proof is adduced to show that sewer-gas destroys unventilated (*i.e.*, not open at the top) pipes in from twelve to twenty years, and ventilated pipes in from twenty to thirty. Since writing on the subject in 1872, I have repeatedly traced typhoid and diphtheria to this source.

[2] And several of them ventilated by being at the top.

As regards the first point, I may say that almost all the decayed pipes on the table were from houses where the drains were trapped between the house and the sewer. In one case I detected this state of pipes as a cause both of typhoid and diphtheria in an establishment where the party in charge specially congratulated himself on the security of his traps. He showed me them with no little satisfaction, and I assure you they were the most effective traps I have seen. Yet in that very establishment, four years ago, it cost upwards of £100 to renew the plumber-work thus corroded; and I am happy to mention that there has not since then been a single case either of typhoid fever or diphtheria.

As regards the second supposition, I need hardly indicate how little probability there is of decomposition going on in the trap, to such an extent as to produce sufficient gas to occasion the destruction of lead pipes. We are, therefore, shut up to the last alternative —viz., that however well drains may be trapped, sewer-gas will find its way from them into our houses;[1] and any one who is acquainted with Graham's investigations as to the diffusion of gases, will readily understand how this will happen. At the suggestion of my friend Mr Scott, this has been illustrated as follows:—



A bent tube such as the annexed was used, *e* representing house side of trap or soil-pipe; *b*, sewer side of same pipe; *c, c, c, c*, level of water in trap; *d*, small vessel or test-tube, from which the gas was evolved; *e*, position of the test employed; *f*, position of tube subsequently added to ventilate the trap.

In the first experiment a small glass tube was placed at *d*, containing solution of ammonia; in fifteen minutes it passed up the tube, through the water, and discharged the acid, with which litmus paper had been reddened, suspended at *e*.

In either experiment, instead of litmus paper at *e*, a small test-tube containing a little Nessler solution was introduced; in half an hour the reaction of ammonia was very distinctly visible.

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[1] Dr Angus Smith found that 8000 cubic feet of the air of a house, into which sewer-air had penetrated, destroyed more than twenty times as much potassium permanganate as the same quantity of pure air.

The tube was left suspended by metal wire all night, and in the morning the wire was eaten through, and the test-tube had fallen to the bottom.

An objection was taken to these experiments because ammonia is lighter than air, and has a strong affinity for water. Other gases, heavier than air, were then tried; the results were much the same, as will be seen from the table now given:—

Gas.	Sp. gr.	Source.	Test.	Time for reaction to show.
Ammonia......	·50	Solution	Litmus.....................	15 minutes.
Do.,	do.,	do.,	Nessler....................	30 minutes. Eat through a small wire in less than 24 hours.
Sulphurous acid..........	2·25	do.,	Litmus....................	1 hour.
Sulphuretted hydrogen...	1·25	do.,	Lead paper...............	3 to 4 hours.
Chlorine.......	2·50	do.,	Iodide of starch paper,	4 hours.
Do.,	do.,	do.,	Litmus water in trap,	Began to show in a few minutes. In half an hour the whole was bleached.
Carbonic acid,	1·50	Generated	Lime water in trap.....	1½ hours.
Do.,	do.,	do.,	Litmus suspended at *e*,	3 hours.

Against these experiments, it was urged that the result would be different if the trap were ventilated. To meet this objection, they were repeated with the trap, somewhat modified by the introduction of a ventilating-pipe, which is represented by the dotted lines in the figure. The results were much the same, except that the reaction was a little longer in showing itself.

Mr M'Tear, F.C.S., repeated and varied the experiments, and the results will be found in the Appendix to this paper, to be published in the March number of this Journal.

Possibly, some may think that I am dwelling on this subject at greater length than its importance demands; but we must all be roused from the false security of thinking that we can purchase safety by well-trapped drains.

Under my fifth head, I now remark that there has never been any evidence brought forward to prove that the state of the river Clyde, though often offensive, is in reality injurious to health. Diseases referable to excremental pollution do not prevail to a greater extent on its banks than in other parts of the city.

I wish I had time to read to you an extract from Dr Rennie's work, on "Pekin and the Pekinese," in which he clearly proves that decomposing matter, when left freely exposed to the air, ceases to be injurious to health. When in possession of Pekin, we began to improve its sanitary condition, but the results were so unsatisfactory, that we were obliged to return to the ways of the heathen

Chinee. If this is correct, we should pause before we expend a vast sum of money on the execution of some grand engineering scheme, such as that of Messrs Bateman and Bazalgette, which might purify the river, but, as the sewage would be pumped, would also, in all probability, be as injurious to the public health as the same process has been in other towns.

I have a great respect for Mr Bateman, and am sorry to find that in the 1868 report there is a departure from the sound views contained in the joint report of Dr Anderson and himself, published in 1858. Possibly, in 1868, his better judgment was warped by his coadjutor, or overshadowed by the main drainage scheme for London; the success of which, in a sanitary point of view, may be tested by the following quotation from the last edition of Dr Murchison's work on Fevers, published this year. He says, "It is not a little remarkable that the increase of enteric fever in the metropolis has been contemporaneous with the completion of the main drainage scheme."

If I have succeeded in proving to you that sewer-air is injurious to health, and that it cannot be kept out of our houses, you will naturally ask me what is to be done. Are we, with Oriental fatalism, to sit down with our hands folded, and say God wills it? If such had been my conclusion, I would not have wasted your time here to-night.

It appears to me intolerable that health should be lowered and lives wasted when there may be a remedy; and I often wonder if the community will ever recognise the fact, that every case of typhoid (enteric or gastric) fever is owing to our taking into our systems the results of excremental decomposition, by breathing air or drinking water tainted by it. Besides the positive injury proceeding from our present system, there is its wastefulness, Mr Smith having shown us last year, in a very vivid manner, that its agricultural waste is equivalent to sending thousands of loaves down the Clyde.[1]

Let us now turn to what we know of remedial measures. If a future Mr Buckle shall ever write the history of human folly, as was projected by Mr Caxton, the various sewage patents that have been taken out will afford him ample material for an interesting chapter.

Sewage has been tortured in every possible way, and all kinds of materials have been employed to precipitate it, each new scheme being heralded by a vast flourish of trumpets as the solution of the difficulty, and as a perfect mine of wealth to the fortunate shareholders. I need not tell you that most of them have, of necessity, ended in failure as to the results *quoad* sewage, and disappointment as regards the shareholders. I use the term "necessity" advisedly, because most of the projectors were ignorant of the very complicated nature of the fluid they had to deal with, and fancied that, if they could precipitate the suspended and part of the soluble matters, they had solved the difficulty. Time will not permit me to enter into details as to these schemes; and it is not necessary, because they all propose to deal with sewage at the outfall, after the mischief has been done, involving all the evil effects that are produced by the decomposition of sewage.

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[1] If we are shocked by the waste of bread, how much more should our feelings be roused by the consideration that our present sanitary arrangements occasion directly the death of hundreds of our population just as effectually (but more leisurely and cruelly) as if we administered to them a dose of some potent poison.

No plan can at all meet the exigencies of our case which does not arrange that all decomposition in the sewers shall be prevented.

No doubt we shall meet with many difficulties in so great an undertaking, but I know of no subject for the sake of which we should face them more determinedly; and I am persuaded, if we did so, our persevering efforts would be crowned with success. Many of you are aware that some time ago the Board of Police sold the urine from the public urinals to Mr Chapman, and that he has been collecting it for some time by pneumatic pressure, and subjecting it to chemical action. To do so with the whole of the excreta of Glasgow would be a vast undertaking indeed, but I am thoroughly convinced that in some such direction we must look for the solution of our problem. Experiments have been made already as to the practicability of doing so; and leaving the water-closets to all appearance just as they are, the Board of Police fitted up what are called Hoey's closets in a set of workmen's houses to test this. The closets *appear* exactly as at present, the difference affecting only their interior arrangements. The flush-pipe is not attached to the general cistern, but to one holding only one-fifth of a gallon of water, which takes two minutes to fill. When the bar of the water-closet is raised, the supply to this small cistern is cut off, so that it is almost impossible to waste water; it could only be so by some one raising the bar every two minutes. Then the waste-pipe from the closet flows into a reservoir underground, from which the excreta are removed by pneumatic pressure, without the slightest offence, and then taken to the country, to be evaporated into portable manure.[1]

To this system it is sometimes objected that one-fifth of a gallon will not keep a closet clean; but in reply I may mention that, more than four years ago, I had a closet fitted up on this principle, and it has kept quite as clean as other closets in the same house where the supply is unlimited. Another objection urged is, that the sewage matter in the tank will decompose. This, however, has not been found to be the case.

The Board of Police requested Dr Wallace to make an exhaustive analysis of this and other processes, and I shall take leave to quote what he says about Stanford's carbon process:—

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[1] The lithograph attached to the paper fully explains the mechanism of Mr Hoey's system.

"The process of Mr Edward C. C. Stanford, of the British Seaweed Company, for the utilization of excrementitious matters by means of seaweed charcoal, has been before the public for some time, and is now well known. This variety of charcoal has a wonderful power of absorbing gases—in other words, deodorizing—and is, I think, better adapted for this purpose than any other variety of charcoal, not even excepting that made from bones. There are, I understand, several large workshops where necessaries on Mr Stanford's plan are in use, and they are said to be eminently successful.

"The following is the composition of a sample of the product of Stanford's closet system, received on the 16th ult. from Mr Ellison:—

	Dry, per cent.	Original.
Organic matter,	11·16	3·74
Carbon,	37·68	12·65
Phosphate of lime,	3·54	1·19
Sulphate of potash,	3·07	1·03
Chloride of potassium,	·70	·23
Chloride of sodium,	1·74	·58
Sulphate of lime,	1·43	·48
Carbonate of lime,	19·35	6·50
Carbonate of magnesia,	3·92	1·32
Oxide of iron,	3·61	1·21
Alumina,	4·12	1·38
Silica,	9·68	3·25
Water,	...	66·44
	100	100
Nitrogen equal to ammonia,	3·70	1·24

"The various ingredients in this mixture are partly those of the charcoal, and are partly due to the urine and fæcal matter, but the composition will vary with the length of time the charcoal has been in use, and the number of times it has been regenerated. In this system the valuable ingredients of the excrements are entirely preserved, with the exception, perhaps, of traces of ammonia, which will escape if the mixture is kept for any length of time exposed to the air."

Now, these and other schemes show a profit on paper, but I do not think that should be our first object. We ought to take a broader view of the question, and feel that this gigantic evil *must* be got rid of. We must not go on contrary to the laws of nature, lowering our health by breathing impure air, wasting our manure, swelling our death-rates, and robbing posterity.

And now I have to apologize for detaining you so long, but the subject is really all-important; and I am most anxious that Glasgow, which has set such a noble example as regards the water-supply, shall not make a mistake in this equally important matter.

If it is true that organic poisons producing disease may pass from sewage; if it is true that cholera, diphtheria, typhoid fever, and diarrhœa are traceable to the taking into our systems, by air or water, the results of decomposition of human excreta; if it is true that these diseases and others from the same cause swell our death-rate, and carry off some of the most valuable of our population,—then, Gentlemen, I affirm that the only true sanitary solution of our difficulties is, that all excreta shall either be returned to the earth or subjected to chemical action, rendering decomposition impossible; and I am, furthermore, sure that if a tithe of the time, skill, and ingenuity, and one-thousandth part of the money, that have been devoted to water-carriage had been spent in investigations in this direction, the problem of the sewage question would have been solved long ago.

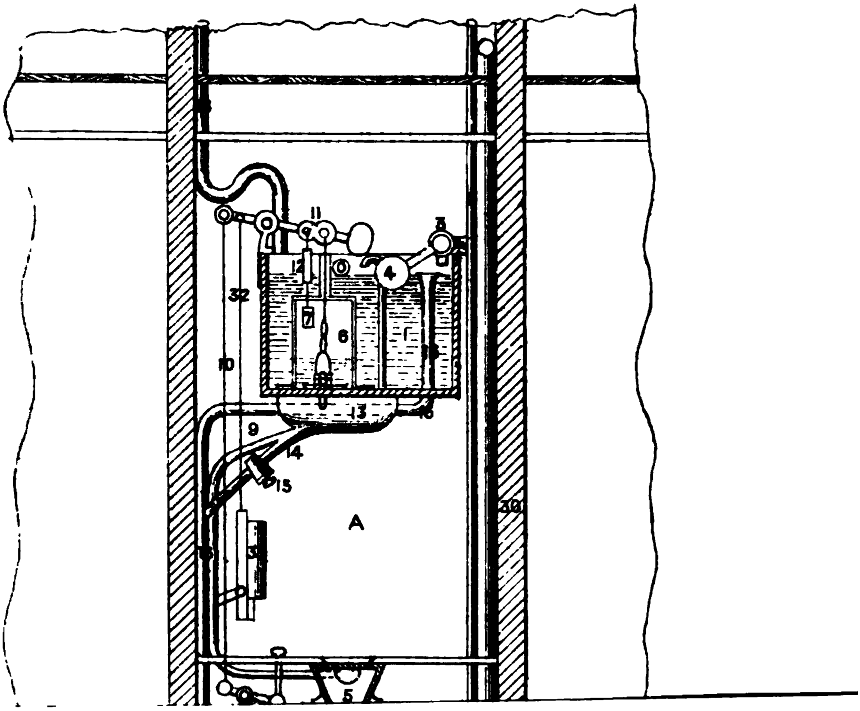
Description of Diagram.

A.—Vertical section of a w.c. in the first floor of a tenement originally fitted with the ordinary piping and apparatus where a pan w.c. is used.

B.—Section of a similar w.c. on ground floor of same tenement, in both of which the cistern (1) is supplied from the main by the pipe (2) through the stop-cock (3), the water in cistern being regulated in height by the ball-float (4).

The limitation and consequent amount in saving of water is effected by enclosing the flushing valve (8) by a cylindrical chamber or metre (6) of any required dimensions, the lower end of which has a flange jointed on and fixed with screws to the bottom of water-cistern (1) so as to be perfectly water-tight. Centrical on the top of the meter or measuring chamber a lead pipe (*o*) is soldered, of such length that the top of the pipe is on a level with top of the cistern sides. The wire for lifting the flushing valve (8) passes down through the pipe (*o*), which also serves as an air-pipe to the meter (6) when being discharged. There is a small hole through the top of the meter as an inlet for water, which serves also to allow of a wire passing down through the same; this wire has a small indiarubber valve and lead weight (7)—within the meter (6)—attached thereto, opening downwards; the top end of the wire is connected to a small indiarubber band (12), and through it raised and lowered by the action of the lever (11), which also, by a wire, lifts the flushing valve (8). During the slipping action of the slack links at the bottom of the wire for lifting the flushing valve (8), the indiarubber band (12) draws up the small indiarubber valve (7), thereby shutting the inlet before the flushing valve begins to lift. The subsequent stretching of the indiarubber band (12) permits the flushing valve to open sufficiently to allow the water to escape at once from the meter (6) into the service-box (13); the contractile force of the indiarubber band (12), when the action of the lever (11) is reversed, ensures the flushing valve being shut before the inlet valve begins to open, and the further motion of the lever (11) opens the inlet valve (7), so as to allow a free ingress of water to the meter. The ordinary overflow pipe (16) shows by the dotted part (31) the usual connexion with the soil-pipe at w.c. cesspool, that portion of the overflow pipe (31) is separated from its connexion with the soil-pipe, and turned downwards so as to discharge the overflow into the w.c. cistern of the house below, having a trap (19) formed thereon to prevent vitiated air, or air containing elementary contagious matter, ascending through it to the houses above. The leakage intercepting pipe (14) forms a connexion between the flushing pipe (9) and the overflow pipe (16), and has a regulating stop-cock (15), which is so adjusted that if the flushing valve gets leaky to the extent of the whole water entering the meter through the inlet valve, all such water would pass through the leakage intercepting pipe (14) directly into the overflow pipe (16), and through it be delivered into the cistern of the house below, and there become available for w.c. flushing; thus preventing the usual, and at present unavoidable, waste of water, and at the same time preventing the further dilution of human refuse collected in the intercepting tank (22), which is placed at, or as near as possible to, the bottom of the soil-pipe (30). In the ground-floor w.c. the overflow pipe is continued down to the level of the street gutter, where it is allowed to escape in such a manner that the inspector, in his ordinary rounds, cannot fail observing it, if there be such in any of those connected w.c.s from the top to the bottom of a tenement, whether caused by float-cock, flushing apparatus, or from any other leakage whatever.

VERTICAL SECTION.



[1] Uber die Hemmungs-mechanism für die Reflex thätigkeit des Ruckenmarks. Berlin, 1863.

The ordinary soil-pipe (30) is made to terminate within the intercepting tank (22), so that the human refuse—which now pollutes our drains and sewers, discharging the poisonous gases generated therein into our street atmosphere, if not directly into our houses—drops innocuously into a close intercepting tank (22), which is emptied of its accumulated contents once a week by atmospheric pressure acting on its surface discharging it through the pipe (25), which has a screwed coupling (34) at the end terminating in the street, for the purpose of forming a connexion with an exhausted tank placed on a carriage, into which it is received and therein conveyed to the depôt. The end of the discharging pipe in the tank dips into a well (26), which is covered by an inverted trough having side apertures (27), to ensure the tank as near as possible being completely emptied at each discharge into the exhausted carriage tank, the exhaustion of which is effected by the action of two air-pumps while on its return journey when empty. At the further end of the tank (22) there is a trapped overflow (24), which, in case of neglect or accident, would allow any redundancy of liquid to escape into the ordinary drain-pipe (23). This trap prevents air from passing into or out of the tank except through the wire cage (29), which is filled loosely with seaweed charcoal, and fixed on the top of and communicating with the overflow pipe (24), through which the air required for discharging the contents of the tank must pass. The dipping rod (28) is used for ascertaining the depth of the tank's contents at any time, but especially immediately before emptying, for the purpose of computing the quantity of human refuse and the water to be discharged therefrom. The disinfecting apparatus (33) is made to act, each time the closet is flushed, by the action of the wire (32), and discharges a given quantity of acid along with each flushing, which, in its descent, is mixed with the flushing water; the displacing plunger is moved by the suspending wire (32) attached to the lever (11), which is moved by the wire (10), the lower end of said wire being connected to a second lever moved by a third lever which gives motion to the closet-pan when the handle is pulled up.

Since Whitsunday 1872, there have been seven of these closets in uninterrupted use, as an experimental test, at the instance of Glasgow Police Board.

Several members of the Board having expressed a doubt that, although these closets might do where each family has its own closet, they might not suit where two, three, or four families used the same closet,—to test this opinion, a tenement was selected, consisting of four stories, with four houses in each flat—the occupants of the said four houses using one common closet, erected on the stairhead. These closets were altered to the limited flushing plan, and have been in operation since Whitsunday 1873, with perfect success, requiring no more attention from the persons using them than when working on the unlimited flushing plan, with this advantage, that there is now no smell in any of the closets, whereas formerly there was.

The disinfecting apparatus (33) has not been introduced into any of these closets, not having been found necessary; the attachment to and removal of such from any closet is an operation of the simplest kind, and could be accomplished at any time.

The tanks are emptied once a week; the contents may be mixed with the city ashes, or, as at present, be delivered at the Sewage Company's Works, and operated upon in the same manner as the urine collected by the company from the city urinals. The specific gravity of the urine is 1020, while the products of these closets have a specific gravity of 1011.

In all these closets the quantity of water used for flushing is about one-fifth of a gallon. The process of flushing can only be repeated every three minutes. The handle of the closet cannot be fixed so as to make a continuous run.

Any ordinary water-closet can be converted into Hoey's water-closet at a moderate expense, and the alteration makes no change in the mode of using the closet or in its outward appearance.

ARTICLE VII.—PHYSIOLOGICAL PAPERS. No. II.—*On the Inhibitory or Restraining Action which the Encephalon exerts on the Reflex Centres of the Spinal Cord.* By JOHN G. M'KENDRICK, M.D., F.R.S.E., Physiological Laboratory, University of Edinburgh.

THE question as to whether the grey matter of the brain, under any circumstances, inhibits or restrains reflex actions in the spinal cord, has been investigated by several Continental physiologists, who have, however, differed in their conclusions. Setschenoff[1] was, so far as I am aware, the first who asserted that the grey matter of the brain of the frog inhibited the reflex action of certain centres in the spinal cord. The same was asserted to be the case in dogs by another inquirer named Simonoff.[1] An elaborate experimental inquiry was made on the frog by Paschutin,[2] with the following result: irritation of the hemispheres of the brain diminished the tendency to reflex action produced by placing a drop of acid on the skin, while the tendency to reflex action excited by gentle titillation appeared to be increased. On decapitation, the tendency to reflex action excited by titillation was diminished.

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[1] Uber die Hemmungs-mechanism für die Reflex thätigkeit des Ruckenmarks. Berlin, 1863.

On the other hand, Herzen,[3] after a series of experiments in Schiff's laboratory, was obliged to come to an exactly opposite conclusion—namely, that there is no such inhibitory action.

The theory of an inhibitory action of certain nerves over reflex nervous centres which act automatically may be now considered as established in several instances, such as (1) the action of certain fibres of the vagus on the ganglia of the heart, (2) the action of the chorda tympani partly as a vaso-inhibitory nerve, diminishing arterial tonus—that is, the normal amount of contraction of the vessels—in the sub-maxillary gland, or (3) the action of a portion of the splanchnic nerves as affecting the heart and the vaso-motor centres of the intestines.

I have repeated many of the experiments made by the observers above quoted. The difference stated by Paschutin as existing between the tendency to reflex action produced by acids irritating the skin and that caused by gentle titillation has not been observed. The method of inquiry I adopted was, in the first instance, to observe the amount of reflex action produced in an animal anæsthetized when an area on the skin was excited by an electrical stimulus of definite strength, and afterwards to note whether the same amount of reflex action was obtained by the same strength of stimulus, applied to the same place, during electrical stimulation of the surface of the cerebral hemispheres. The results of this method were far from being satisfactory. Sometimes, during stimulation of the surfaces of the hemispheres, reflex action was weaker, but as frequently no effect was observed.

Another mode of experiment was adopted, which satisfied me that when a strong nerve-current is transmitted down the spinal cord, reflex action may be restrained or inhibited. Instead of detailing many experiments, it will better suit the purpose of this article to give an illustration of the method. On decapitating a pigeon (after it has been chloroformed), the body lies for a second or two comparatively still. There is muscular tremor in all parts of the body. As the cord loses its supply of blood, the muscles become violently convulsed. With outspread and quivering wings, the convulsions are frequently so great as to cause the body to throw backward somersaults. Now, if, during these convulsions, the body be held loosely by an assistant, and a current from the secondary coil of an induction machine, sufficiently strong to be felt acutely by the tip of the tongue, be applied to the upper end of the cord so as to stimulate it, the convulsions diminish in violence, and may be even nearly arrested. On removing the stimulus, the convulsions recommence. In this experiment, the electrical stimulation of the upper end of the cord excites nerve-currents, which pass downwards through the cord, and these currents apparently restrain the action of the reflex centres. To succeed in the experiment, the electrical stimulation must neither be too strong nor too weak. If too strong, there will be a violent convulsive attack which exhausts the nervous energy of the cord at once; if too weak, no effect will be observed. This is in accordance with Professor Lister's observation, that the degree or amount of stimulation affects the result in all experiments on nerves.

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[1] Reichert's Archives, v. 545.

[2] Centralblatt, 1865, p. 794.

[3] Beale's Archives, 1867.

Another kind of experiment I have made, which is even more satisfactory. There has been a dispute among experimental physiologists as to whether or not the columns of the spinal cord can be stimulated directly in any way, by electrical, chemical, or mechanical agents? The statement in the affirmative is supported by Fick[1] and Engelken;[2] while it is denied by many other authorities, such as Van Deen, Schiff, Guttmann, Funke, Sigmund Mayer,[3] Huitzinga,[4] and Aladoff.[5] Aladoff asserts that the posterior columns are irritable while the anterior are not so. The matter is one which is not so easy to settle as might be imagined—indeed, the array of names known in physiology, which I have quoted, at once indicates a difficulty. It is possible I may have omitted the names of others who have investigated the matter. The difficulty is twofold: (1) on applying electrodes to the anterior columns, the anterior roots of the spinal nerves may be directly irritated and muscular contractions will consequently take place; and (2) on applying electrodes to the posterior columns, the posterior roots may be directly irritated so as to produce muscular contractions by reflex action. In neither of these instances can it be asserted that either the anterior or the posterior columns of the cord have been directly stimulated. In this controversy, I must place myself with the minority. By using very fine electrodes so as to touch only a minute portion of the cord midway between the points of entrance of the roots of the nerves, muscular movements may be induced, not always in the part supplied by the nerves near the electrodes, but in parts supplied by nerves issuing from the cord posterior to the point of stimulation.

Being satisfied that it is possible to stimulate the cord directly, the experiments were then made, of which the following is an example:—A pigeon under the influence of chloroform was decapitated, and with as great rapidity as possible the posterior wall of the vertebral canal was removed so as to expose the posterior surface of the spinal cord down as far as the seat of the reflex centres for the legs. Two electrodes of fine wire were placed over the seat of the reflex centre for the wings, and this centre was irritated by an induction current from a Du Bois-Raymond's electromotor, so weak as just to cause slight flapping of the wings. Two similar electrodes were applied to the upper end of the cord. This second pair was in connexion with another similar electromotor, and by moving the secondary coil backwards or forwards, a current slightly stronger than that of the first electromotor was obtained. It was then found that on stimulating the upper end of the cord, the flapping movements of the wings were arrested, which were produced by stimulating the reflex centre (or, at all events, the sensory filaments in connexion with this centre). This proved that the nerve-current, set in action by electrical stimulation at the upper part of the cord, and then transmitted down the cord, was capable of restraining reflex nervous action produced by a stimulation of the posterior columns, or by stimulation of filaments derived from the posterior roots of the spinal nerves.

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[1] Pflüger's Archives, 1869, p. 414.

[2] Reichert's Archives, 1867, p. 189.

[3] Pflüger's Archives, 1868, p. 167.

[4] Pflüger's Archives, iii. p. 181.

[5] Bulletin de l'Academie de St Petersbourg.

In the spinal cord, therefore, there are nerve-tubes conveying influences which restrain the action of reflex centres in that organ. The part of the encephalon from which these nerve-tubes originate I do not know. It may be situated in the grey matter of the cerebral hemispheres. In birds, faradization of the grey matter of the cerebral hemispheres is followed by no muscular movements, so far as I could discover, after numerous experiments, except (*a*) movements of the eyeballs, (*b*) movements of the iris, and (*c*) movements of the lower jaw. This fact has also been pointed out by Ferrier.[1] There are good grounds for believing that many of the motions of birds—such as the movements of the wings during the long flights of migratory birds—are of a purely reflex character. We cannot imagine that each act of flapping the wings is preceded by volition. The successive movements of the wings, exactly similar to each other, depend on an automatic mechanism. But this mechanism may be quickly restrained, in the way I have indicated, by nervous currents passing from some part of the brain to the reflex nerve-centre. These observations also assist in the explanation of how we are able, by a strong effort of will, to restrain in a great measure reflex actions excited by tickling. These movements are reflex, because they may occur (1) during the unconsciousness of sleep, and (2) after disease or injury of the upper part of the cord so extensive as to prevent the transmission of nervous influences upwards to the brain. During consciousness, and with a healthy and uninjured cord, we feel the sensation of tickling, and therefore there must be nerve-tubes conveying sensory impressions to the brain; other nerve-tubes probably terminate in cells situated in the reflex centre. But, as is well known, even while we continue to experience the sensation, we can, by a strong effort of the will, restrain the muscular action. This is an inhibitory influence on the reflex centre.

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[1] The West Riding Lunatic Asylum Medical Reports, edited by Dr Crichton-Browne, vol. iii. 1873, pp. 33, 34, and 61.

I have shown, in another paper,[1] that one of the most important functions of the cerebral lobes of birds is in relation to vision. This fact is corroborated by the remarkable experiments of Gudden,[2] who has shown that ablation of the organs of vision in birds newly hatched is followed in after-life by a considerable diminution in the size and weight of the cerebral hemispheres. If, in addition to the perception of visual impressions and the evolutions of nervous energy inducing muscular movements consequent thereon, we also place in the cerebral hemispheres of the bird an apparatus for controlling the reflex actions of the spinal cord which produce systematic, regular, and simple movements of the wings and legs, we can readily understand how it is that faradization of the surface of these hemispheres is followed by no muscular contractions. The faradization may actually inhibit the reflex centres, and consequently no movements occur.

Part Second.

REVIEWS.

The Conservation of Energy: Being an Elementary Treatise on Energy and its Laws. By BALFOUR STEWART, M.A., LL.D., F.R.S., Professor of Natural Philosophy at the Owen's College, Manchester. London: Henry S. King and Co.

PROFESSOR BALFOUR STEWART'S work is a lucid and extremely simple exposition of the laws involved in the general statement that energy is indestructible. Beginning with a definition of the meaning of energy, he proceeds to explain the various forms in which it may exist, and the transmutations which occur, if energy turn one form into another. Although the book is rigidly elementary, requiring a knowledge of only the simplest elements of algebraic notation, yet the enumeration of the various energies of nature is complete, and sufficiently elaborated to give a clear conception of the meaning of the two great doctrines of the conservation and dissipation of energy, which teach that, while no force in nature can create or destroy energy, yet that the energy available to produce change is gradually and necessarily diminishing; two doctrines which imply that the universe since it was first subject to existing laws has steadily decayed, and will decay, until a deathlike and eternal uniformity prevails.

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[1] Observations and Experiments on the Corpora Striata and Cerebral Hemispheres of Pigeons, communicated to the Royal Society of Edinburgh by Prof. Turner, on 6th January 1873.

[2] Experimentaluntersuchungen über das peripherische und centrall Nervensystem. Archiv. für Psychiatrie und Nervenkrankheiten, Bd. ii. p. 693.

Nothing can be sounder than that portion of the work which treats of the "Position of Life" in the scheme of energy. Our author does not travel one step beyond the limits of positive science. He suggests, indeed, unless we are mistaken, that free will need imply no contradiction of the physical laws of energy, since in the language of mechanical science the will may be conceived as a *deviating force* which does no work; but while he supplies the materials from which this conclusion may be drawn, he as carefully refrains from drawing the above inference as he does from upholding the conception of man as a mere machine, incapable of independent volition transmitting to one set of members impulses which are received through others.

The book is written in a very familiar style; perhaps here and there too familiar. A lecturer by grace of manner may make a little humour acceptable even if its quality is not very high, but in writing on physical subjects it is doubtful whether the semi-humorous tone here and there adopted by our author can ever be agreeable. This blemish, if it be one, is very small, and the book may be cordially recommended as useful and interesting to very many classes of readers. F. J.

Animal Locomotion, or Walking, Swimming, and Flying; with a Dissertation on Aeronautics. By J. BELL PETTIGREW, M.D., F.R.S. London: H. S. King and Co.

THIS is the most recently published volume of the *International Scientific Series*, a set of works intended exclusively for popular instruction.

There are two qualities which a popular scientific work ought to possess, and without which a work cannot be either scientific or popular. The first is *precision* in the use of language, and is absolutely requisite to the very existence of science. The second, often, but not necessarily, secured by the presence of the first, is *intelligibility*. Most readers are competent to judge of the presence or absence of the first; any one of average intellect may be trusted to test the presence or absence of the second. We therefore consider ourselves competent to give a definite verdict, and, in the case of each of these qualities, our verdict on Dr Pettigrew's work is, "*Wanting.*"

We will presently give extracts sufficient to justify these conclusions; but, before doing so, we are bound to say what we can on the other side—and that is not little, though it can easily be compressed into a single sentence or two. Dr Pettigrew is obviously a dissector of great ability; a keen and accurate observer, and a skilled draughtsman; some of his illustrations, indeed, are admirable. But he does not seem to be aware either of the difficulty of the subject he has attacked, or of the absolute necessity for an extensive and accurate knowledge of mathematical and physical science to any one who attacks it. This is the secret of his failure. When men like Helmholtz, who have all his anatomical knowledge, and, besides, possess perfect mastery over all known physics and mathematics, recoil from attempting the question in its generality, it is not surprising that Dr Pettigrew should fail. He is attempting, as it were, to climb the Matterhorn without guides, ropes, or even an ice-axe; and, under such circumstances, with merely the ordinary means of locomotion, success is impossible. But a man who attempted such a thing would deserve censure for his foolhardy attempt, not compassion for his failure; and the more strongly, because his example may induce others to risk their lives in the same ridiculous manner.

A man who has read Dr Pettigrew's book will probably know a good deal more about locomotion than one who has not, but he will owe his knowledge mainly to his own inferences from the excellent woodcuts, not to the author's comments on, or explanations of, them: for these, from their excessive looseness, are simply bewildering.

We give a few extracts, not affected in meaning by the context, to justify what we have said. The italics are our own:—

"It is a remarkable circumstance, that the undulation or wave made by the wing of an insect, bat, or bird, when those animals are fixed, or hovering before an object, and when they are flying, corresponds in a marked manner with the track described by the stationary and progressive waves in fluids, and *likewise with the waves of sound.* This coincidence would seem to argue an intimate relation between the instrument and the medium on which it is destined to operate—the wing acting in those very *curves into which the atmosphere is naturally thrown* in the transmission of sound."

What the author's notion of the form of a sound-wave may be, cannot be clearly gathered from this very remarkable comparison. That that notion, whatever it may be, is utterly erroneous, admits, however, of no doubt.

"Of all animal movements, flight is indisputably the finest. It may be regarded as the poetry of motion. The fact that a creature as heavy, bulk for bulk, as many solid substances, can, by the unaided movement of its wings, urge itself through the air with a speed little short of a cannon-ball, fills the mind with wonder."

This is rather more than startling. It is simply the assertion that some birds can fly at a rate approaching to a *thousand* miles per hour. The same thing is stated in a different form of words a little later, where we find—

"Flight, because of the rarity of the air, is *infinitely* more rapid than either walking, running, or swimming."

Here is something quite new in physics:—

"A fish, or sea-mammal, is of nearly the same specific gravity as the water it inhabits; in other words, it is supported with *as little* or less effort than a land animal."

Mathematicians will wonder, perhaps, how it can be that "fishes are for the most part *elliptical* in shape;" and also how "a biped . . . . may be said to roll over its extremities, the foot of the extremity which happens to be upon the ground for the time forming the centre of a circle, the radius of which is *described* by the trunk in forward motion." What on earth is a "twisting diagonal movement" or a "spirally sinuous" one?

Again, we are told that "another and very obvious difficulty is traceable to the great disparity in the weight of air as compared with *any known solid*, this in the case of *water* being nearly as 1000 to 1;" and, shortly afterwards, that "the *boasted levity* of insects, bats, and birds, concerning which so much has been written by authors in their attempts to explain flight, is delusive in the highest degree."

We come now to a passage which shows that, in our author's opinion at least, Sir Boyle Roche was not very far wrong when he said, "I'm not a bird; I can't be in two places at once!" It is as follows:—" . . . . the space mapped out by the wing during its vibrations *is entirely occupied by it; i.e.*, the wing (such is its speed) is in every portion of the space at nearly the same instant, the space representing *what is practically a solid basis of support*."

The author's remarks on the sounds produced by the wings of insects, birds, etc., are calculated still more to astonish a reader who knows anything of physical science. Pages 120–122 of the work are really wonderful, but we cannot reprint them in full, and to make extracts would be only to spoil a consistently preposterous whole.

We must soon close this very short notice of what is at first sight an entertaining, but on reflection a very painful, book. We know the fate that awaits us, should the author deign to notice our remarks. He has not spared even Professor Marey, and the following speaks for itself:—"Captain F. W. Hutton . . . . refers to some of the experiments detailed above, and endeavours to form a theory of flight, *which differs in some respects from my own*. His remarks are singularly inappropriate, and illustrate in a forcible manner the old adage, 'A little knowledge is a dangerous thing.' If Captain Hutton had *taken the trouble to look into my memoir* . . . . communicated . . . . fifteen months before his own paper was written, there is reason to believe he would have arrived at very different conclusions. Assuredly he would not have ventured to make the rash statements he has made, the more especially as he attempts to controvert my views, which are based upon anatomical research and experiment, without making any dissections or experiments of his own."

The gist of what Dr Pettigrew evidently regards as his greatest claim to discovery is "the figure-of-8 wave movement." An observation made by him, and a little later by Marey, by means of a method long ago used by Dr Young, shows that each point of the wing of an insect (whose body is fixed) describes a curve somewhat resembling the figure of 8. He endeavours to extend this throughout the animal kingdom; and his mode of doing so, as regards the horse, bull, etc., is particularly rich. He seems to have no notion that a part of an elongated figure of 8, *because it contains a point of inflexion*, secures the nearest approach to straightness which can be made by a necessarily curved line. We would recommend him, before he writes any more on the subject, to study Watt's *Parallel Motion*. Here he will find his figure of 8, and may perhaps get some notion of the reasons for its occurrence.

In conclusion, we can only repeat, in other words, the substance of our first remarks—that it is really lamentable to see, usually in men of special note in some very limited line of inquiry, the almost complete ignorance exhibited as to subjects which ought to be, to some extent at least, familiar to every educated man. The recent rapid progress of physiological research has been almost entirely due to advances in chemistry and physics; and no one need expect to become a discoverer in such a field, instead of at best a mere carrier-out of the ideas of others, or at worst a disseminator of nonsense, who has not mastered a sound knowledge of these indispensable auxiliaries.

A Clinical History of the Medical and Surgical Diseases of Women. By ROBERT BARNES, M.D., etc. London: Churchill: 1873.

GRAILY HEWITT had better be on the alert, for here is Barnes manifestly bent on poaching on his manor. He gives us a larger volume than Hewitt's, which adds weight to what he says; and he has the manner of a schoolmaster towards all who differ from him. He is always laying down great principles and theories, and, as a sample of them, we give those in relation to the greatest displacements of the uterus. "The leading fact," says he, "in the history of prolapsus of the uterus, is imperfect involution after labour." . . . . "The theory of prolapsus and procidentia uteri may be summed up as follows:—Invagination or intussusception of the uterus is prolapsus; complete inversion of the vagina or hernia uteri is procidentia." As he goes on with his long pages on displacements, we find little attention paid to the leading fact, which does not lead, or to the theory, which is no theory at all. In the course of his book, generally, Barnes kindly names or praises all his friends, and this, no doubt, they will duly appreciate; but in his chapters on displacements we find the chief authors omitted, probably being not particularly his friends. Especially we find Hewitt, the advocate of the displacement theory, scarcely gets mention, and the late Simpson treated most cavalierly. Others, friends, are mentioned for no reason whatever except that indicated. Simpson is managed otherwise. It was he, we all well know, who made the displacement theory: it was his intrauterine instruments that formed its practical basis. Before his much-lamented death, the theory suffered decadence; the treatment became, comparatively speaking, almost extinct. All his genius and energy could not keep it on its legs, in Edinburgh, Paris, or London. But Hewitt has revived it, and now Barnes comes on. As we have said, he leaves Hewitt out in the cold, and then he treats Simpson as follows:—His instruments are all wrong; don't answer to the true principle; do harm. Everybody's plans and instruments are, of course (and here we agree with Barnes), all wrong. Only the Barnes principles and plans are right. Impale the uterus, but respect its mobility; look at my figure, 133, and go and do likewise. We are, however, not convinced; and we venture to whisper to Dr Barnes that he will have no more success than Simpson—not nearly so much in some ways. He will, though he respects the mobility, have plenty inflammations, and deaths too; and his laughably homœopathic dose of pessary galvanism will not medicate with virtue the intrauterine stems. Barnes's book is full of the gravity of even slight degrees of inflammation. Does he think his vaginal rest will not irritate? Does he think his impalement will not irritate and inflame? His practice in displacements is irreconcilable with his chapter on metritis.

Retroflexion produces pelvic distress, dysmenorrhœa, menorrhagia, uterine hæmorrhage, leucorrhœa, abortion, sterility, ball-valve obstruction of rectum, ribbon-like stools, pain in defecation, accumulation of fæces, permanent distention of the canal, loss of peristaltic power, intestinal paralysis, retrograde obstruction, and disturbance of the whole digestive functions, flatulence, copræmia, fermentation, pyrosis, nausea and various phenomena, disorder of liver, imperfect nutrition, disordered secretions, hysteria in all its manifold eccentricities, sciatica, lumbago, tic doloureux, rheumatism, headache, vertigo, syncope, emotional, moral, and intellectual disturbance, pain in the spine, etc., etc., etc. So says Barnes, and of course so does he manage his practice. It is in vain to ask this enthusiast, so attached to principles, how it happens that many cases of displacement, most perhaps, have no disease or symptoms at all; how the greatest sufferers, with the womb hanging between their legs, frequently have little complaint but of mechanical inconvenience; how even a retroverted or retroflected gravid uterus often produces scarcely a symptom or inconvenience. Barnes is, as we also are, no doubt a great admirer of practical men, and of Trousseau among them; and we tell him that, in London and all over the country, nine practical men out of ten have no faith in his principles of, or practice in, retroflexion. We leave this subject, with a text from his friend Trousseau—"In a still more emphatic manner," says this clinical author, "I denounce intrauterine pessaries, which ought not to be employed under any circumstances; this opinion I the more hold to, from having been taught by experience, that deviations of the womb are almost never the cause of the pains and discomforts from which women suffer. These pains are generally symptomatic of metritis, uterine catarrh, or chronic pelvi-peritonitis, which are only aggravated by the introduction of a foreign body into the uterus." It is the fashion now to have three editions out in hot haste after one another. The displacement chapters will, we hope, be re-written after study of recent works on the subject, and with Trousseau's passage as a warning finger-post before the author.

We have referred to metritis, and are by it reminded of a similarity between the work of Courty and that of Barnes. Like Courty, our author treats this subject in a quite unpractical way. If our readers want an example of fireside, not bedside, description, let them read of the symptoms, diagnosis, and treatment of the following diseases: fluxion, hyperæmia, congestion, inflammation of the uterus, and then of many kinds of this last inflammation. Almost all the writing on this subject is, we confess, beyond our comprehension. Only we find out that, apart from details of treatment, the one word "Rest" gives the leading principle, and rest means that refuge of the destitute—a Hodge pessary. It is always easy to send the patient away with one of these inside. Besides this, you may resort to a small bleeding from the arm, or by leeches from the temple or nucha—this for fluxion, observe! Besides, we have epispastics, blisters, fomentations, purgatives, dry-cupping to the breasts, acetate of ammonia, nitrate of potash, tartarated antimony, aconite, digitalis. In the higher degrees of disease, we have leeching the uterus, scarification, puncturing, and a large list of other remedies.

The chapter on intrauterine medication indicates considerable ingenuity of an easy kind. There is much in it, and almost all of a peculiar tendency that should be discouraged. Its object seems to be, not so much to describe appropriate or judicious treatment of any disease, or set of diseases, as to give an account of numerous applications that may be made to the interior of the body of the uterus without certainly entailing injury or great danger on the patient. The list might have been increased a thousandfold, and yet the result would have been the same.

Hitherto we have been dealing with matters on which opinions do notoriously differ. We now refer to another important part of Barnes's work, where we are in a more wholesome region,—that of facts. The book begins with an anatomical and physiological account of the organs whose diseases are to be described. We cannot say we admire this part; and we wish our author had omitted it, satisfying himself with referring to such admirable works as that of Arthur Farre in the "Cyclopædia of Anatomy and Physiology." Some of the best researches are omitted, and, consequently, much error committed. The valuable researches of Schröne, Pflüger, Waldeyer, Fox, and others, on the ovary and on its diseases, are either omitted or misstated. The researches of Schroeder regarding the hymen are omitted, and the well-known anatomy of the part is very erroneously given,—not to speak of *carunculæ myrtiformæ* (*sic*). As we are at Latin, we may mention another error: in describing a *rete mirabile*, which does not exist, and, not content with this error, he gives us *retæ mirabiles* (*sic*). We observe, also, *hygroma cystica perinealea* (*sic*); but this may be Luschka's, not Barnes's latinity. He misdescribes the hydatid of Morgagni, and discordantly with himself in another passage. The ganglia of the uterine nerves are not described. He speaks of corpora cavernosa of the penis. He exaggerates Cowper's gland to the size of an almond. He describes the glass-like mucus of the cervix, when in excess, as the result of inflammation. We need not give more examples to illustrate this kind of imperfection.

The book is dedicated to the great obstetrician and gynæcologist of Dublin, and the author of a valuable clinical work. The work of Barnes is called a clinical history, but it is in no respect a clinical treatise. It is a kind of systematic treatise, with anatomical and physiological chapters, and full everywhere of superabundant theory and hypothesis. In literary character it resembles the author's "Lectures on Obstetric Operations," but is not nearly so clever, and contains a greater proportion of errors. It was brought on the stage much in the same way as an actor leaves it. Advertised for years, it did not appear. Then it was announced in the bills as nearly ready, or next month; but it did not come. At last it did see light; and we think it might, with advantage, have been kept back a little longer. The advertising might, meantime, have gone on, much in the same chronic way as is adopted by some of Barnes's most eminent colleagues. After perusing the whole book, we are sure that teachers will look with expectation for another edition of West, our best book on diseases of women.

There is much clever writing in Barnes's book; and we end by a quotation, which is so good, and so in consonance with our views, that we reproduce it here. "Another expression," says Barnes, "which is often adopted as a conventional substitute for precise diagnosis, is 'neuralgia of the uterus,' or 'hysteralgia.'" These terms really mean nothing more than "pain in the uterus." To employ Greek compounds to express this idea seems superfluous, unless it be to lull the spirit of inquiry by fostering the false belief that these terms embody a pathological entity. It must not be forgotten that these terms, seemingly so definite, and yet so vague, took their rise at a period when the precise and minute methods of investigation at present in vogue were comparatively unknown. These imposing terms, therefore, are the reflection of imperfect pathological knowledge. They no longer satisfy any but those who are satisfied with the imperfect pathological knowledge of the past. Advancing knowledge has gradually contracted the proportion of cases in which pain cannot be referred to its cause. And with this advance we are less under the necessity of treating pain as an essential disease; we are more able to attack successfully the real disease, of which the pain is a symptom." This passage indicates the philosophical spirit which should pervade all gynæcological writings, and not merely the theoretical parts, but also the practical.

Animal Physiology: The Structure and Functions of the Human Body. By JOHN CLELAND, M.D., F.R.S., Professor of Anatomy and Physiology in Queen's College, Galway. With 158 Engravings. London and Glasgow: William Collins, Sons, and Co.: 1874. Pp. 325.

THE high reputation of Dr Cleland, as a man of science and a teacher, led us, when we saw the announcement of this work, to expect a good book, and in this expectation we have not been disappointed. Forming one of the volumes of Messrs Collins's advanced science series, it is intended "to supply the information required for the Advanced Course of the Directory of the Science and Art Department." This explains why so much anatomical description is met with in almost every page of the book. Dr Cleland writes for those who have not had the advantage of special teaching in anatomy. As it does not contain original matter, but, like most text-books, is a compilation, the work does not call for special criticism. The method of the book is what is usually adopted in a course of physiology delivered to an audience of students. After various introductory matters have been discussed, such as the general scope of physiology, the difference between organic and inorganic matters, the differences between plants and animals (which, we are happy to see, are given briefly, and not with that painful minuteness which many lecturers indulge in, and which weary the student), the meaning of the terms function, nutrition, reproduction, sensation, movement, reflex action, etc., Dr Cleland passes on to consider the various tissues composing the body. The chapter on the skeleton and on skeletal textures, such as cartilage and bone, is very good, with the exception that the description of ossification—confessedly a most difficult subject—will leave any beginner, after having read it, with very vague and hazy ideas as to the steps of the process. The figures illustrating the mechanics of the skeleton are humorous, the second in the series recalling to the memory the figure of an individual now notable in our law courts, while the fourth is suggestive of severe colic. The various kinds of muscle are well described, but the figure illustrating fibre-cells of unstriped fibres represents these structures much too coarsely, and with the nuclei too distinct. The description of the electro-motive force of living muscle is somewhat vague; and the statement that "if, however, the muscle be made to contract, the condition of electric tension ceases," is not correct. When the muscle is made to contract, the needle of the galvanometer never returns quite to zero. When a muscle contracts, the electro-motive force is diminished, but it does not disappear altogether. The next chapter describes the structure and functions of free surfaces covered with epithelium, the phenomena of sensation, and the structure and functions of the skin and of the epidermic appendages. The subject of waste of tissue by action, and the consequent physiological necessity for food to repair waste, is next considered. At page 80 Dr Cleland remarks—"Animals have no power of building up albuminoid matter from simpler chemical substances. They have no power of manufacturing organic materials found in inorganic nature, but feed either directly on the vegetable world, or on other animals which have fed on vegetables; and there is no proof that in any instance they have the power of acting on the simpler organic substances, so as to produce from them the more complex." This statement, though generally true, must be modified so far as certain low animal organisms are concerned. Recent results of dredging in the deep sea have shown the presence of low forms of animal life at immense depths, where not a trace of vegetable life could be found. There, these animal organisms of low type must have the power of directly feeding on inorganic materials. The physiology of digestion is admirably done. Next follows a chapter on the physical and chemical properties of the blood. The views first indicated many years ago by Professor Buchanan of Glasgow, and more recently worked out by Professor Schmidt of Dorpat, regarding the formation of fibrin in coagulation, by the union of fibrinogen and fibrinoplastic substance, are very clearly stated. The two conditions of the colouring matter of the coloured corpuscles, —namely, as hæmoglobin and oxyhæmoglobin—are not alluded to. The chapter on the circulation is very clearly written. Sphygmographic tracings are shown on page 126. No. 3 is not a well-marked example of a dicrotous pulse; and not one of the three can be taken as a good typical example of a healthy pulse tracing. The following chapters of the book, on respiration and animal heat, absorption, the ductless or blood glands, the liver, and the kidneys, present a clear and succinct view of the physiology of the various processes. Fig. 77, showing "Lobules of Human Lung," is, from the peculiar shading, somewhat unintelligible. In the description of the relation of the hepatic cells to the bile-ducts, Professor Cleland states that these "are not to be considered as proper secreting ducts, but correspond with similar channels between the cells contained within the saccules of the salivary glands and pancreas." There is, however, one great distinction between the ultimate structure of such a gland as the pancreas and that of the liver. In the pancreas the secreting cells are within a basement membrane, and have their free borders directed into the duct; whereas, according to the views of Hering, Chrzonszczewsky, and others, the hepatic cells are to be regarded as being outside a basement membrane. The view advanced by Dr Beale, that these cells really line the commencements of the bile-ducts, is supported at all events by analogy. The *résumé* of what is known regarding the functions of the liver, given at pages 167, 168, is the best we have yet read in our language. We have no critical remarks to offer regarding the portions of the book on the nervous system, the senses, voice, and on reproduction. We have in these chapters an admirable epitome of what is known definitely on these subjects. With regard to the vexed question of the presence of any consciousness in the spinal cord, Dr Cleland evidently leans to the opinion that many of the apparent purpose-like movements of a decapitated frog cannot be referred merely to reflex action, and be independent of consciousness. This is a problem in which it is well to be cautious, because certain data necessary for its solution are apparently beyond the region of direct experiment. We have always been at a loss to perceive any real distinction between a so-called *sensorium commune* and consciousness. There can, so far as we see, be no sensation without consciousness. To assert the contrary is to give a new meaning to the term sensation. Any nervous action without consciousness should not be termed a sensation; it is merely certain changes in the nerve or centre, without producing any of the phenomena of mind. In one sense the cord and nerves are necessary to sensation, because they conduct impressions from the periphery to the centre; but the true seat of sensation is in the encephalon. Dr Cleland does not show, what is of great importance physiologically, namely, that brain-function demands a constant supply of pure blood. A due supply of blood is just as necessary to nervous action of any kind, whether reflex or embracing the higher operations of mind, as a healthy condition of the brain-substance. At the end of the volume there is a well-assorted glossary, with derivations, which will be of great service to the student. The book is well got up, and is amply illustrated. Many of the illustrations are from Quain's Anatomy, of which Dr Cleland was one of the distinguished editors. Many are of a diagrammatic character, and may not be readily understood, except by those having some previous knowledge of the subject. In conclusion, we have pleasure in stating that this work is one of the best of the smaller text-books of physiology in our language, and that it will prove of great value to the junior student. Dr Cleland has in this work demonstrated the fact, often observed, that the best elementary text-books are not written by tyros, but by those well versed in the subject.

Don Carlos: A Tragedy. By SCHILLER. Translated into English Blank Verse, by ANDREW WOOD, M.D., F.R.S.E. Edinburgh: W. P. Nimmo.

THE work before us possesses a double claim to a place in the pages of any medical journal. Besides its own intrinsic merits as a brilliant effort of dramatic genius, it has in its present form the additional attraction of being translated by, as well as having been originally the production of, a medical man. Johann Christoph Friederich Schiller, its immortal author, was not only a surgeon himself, but the son of a surgeon, both he and his father having served as medical officers in the army of Wurtemberg; while Dr Andrew Wood, its present *traducteur*, is come of a long line of medical ancestors with which Edinburgh is too familiar to require mention here. The tragedy of Don Carlos is one where all the fiery passion, the deep pathos, and power of poetic thought, characteristic of its author, find ample scope for their introduction and expression. To render in another tongue the ideas and outbursts of a mind like Schiller's, originally expressed in a language such as the German, cannot be done by rigidly adhering to an exact and literal translation of the text. But, on the other hand, no adequate or even faithful and just, perception of the author's meaning or his manner can be realized where the translator, with the desire of making his translation "read," introduces thoughts and metaphors and idioms of his own, and sometimes as original in their way as those from which they have departed were, on the part of the original writer. We have in our own Scotch language, when attempted to be translated, many examples of such difficulty or dilemma, as the case may be. Even in an English rendering of Ramsay's "Gentle Shepherd," published in London in 1790, we, for instance, find the following passage and its translation:—

Bauldy.	*Bauldy.*
"Sae gray a gate! mansworn, an' a' the rest!	"So black away! perjured, and all the rest!
Ye lied, auld roudes! my certie, ye had best	You lie, old runnion! and in faith had best
Eat in yer words, else I sall gar ye stand	Eat in your words, or for this defamation
Wi' a het face afore the haly band.	I'll make you stand before the congregation.
Madge.	*Madge.*
Ye'll gar me stan'—ye shevellin'-gabbit brock!	You'll make me stand! you wry-mouthed drivelling dunce!
Speak that again, an' tremlin' dread my rock,	Say that again, my distaff's on your sconce,
An' ten sharp nails, that, whan my hands are in,	And ten sharp nails, that, when my hands are in,
Can flype the skin o' yer cheeks owre yer chin."	From your false face shall claw the ugly skin.

Here a much more literal translation could have been given, and with much more success in conveying the author's style, not to speak of affording a more correct interpretation—and so it is with some English versions of the greater German poets. The general drift of the author's meaning may no doubt be given, and sometimes be given as well—perhaps better—than in the original. But then it

is in many cases not the author's words, even where the idiom would admit of almost literal translation, and in not a few instances the figurative alteration is quite as great. This is the error which Dr Andrew Wood has studied to avoid; and we can congratulate him on the difficulty being so ably overcome. "In fulfilling my self-imposed task," says he, "I have endeavoured to keep as closely as possible to the original, consistently with the idiom of the language into which it is translated, though I cannot but fear that in too many instances I have failed to steer clear of the tendency to revert to the German idiom. A translator is at times tempted to deviate from literality in order, it may be, to escape difficulties, or it may be from what, as seems to me at least, is the erroneous idea that the duty of the translator is to convey rather the sense than the exact words of the author. But is this fair to the author? Is it fair to the reader, who seeks in the translation the author himself as nearly as possible? I have spared no pains to ensure a correct rendering; how far I have been successful or otherwise my readers must judge."

The story of Don Carlos, as drawn by Schiller, is not to be taken as a correct transcript of that prince's life as he figures in history. Few historic dramas, indeed, are to be considered authentic as biographical sketches of the characters represented. The Don Carlos of the poet is here represented as a noble and high-souled young Prince, of a daring disposition—pure-minded and loveable, but impulsive, and occasionally the victim of wild and ungovernable passions. Ignorant of a mother's love or training—she having died at his birth—his years had passed in the neglect and even dislike of his father, Philip II., a morose, suspicious, vindictive despot.

"Reverend Sir, I've with my mothers had
Misfortunes very great—for my first act
When life I enter'd was to kill a mother.

. . . . .

I know that at this Court I am betray'd—
To watch me that a hundred eyes are bound;
I know King Philip has his only son
Sold to the very basest of his slaves,
And that each syllable caught up from me
Brings to the royal spy such a reward
As for a virtuous deed there ne'er is paid.
I know—but peace! No more of this! My heart
Will burst—already I have said too much."

Such were the relations of father and son at court when the king, now sixty years of age, and already twice married, robbed young Carlos of his destined bride, Elizabeth of Valois—by marrying her himself. Wild, hopeless, friendless—loving her who was now his stepmother with a devoted but, as he felt, guilty passion, Don Carlos is found by the friend and counsellor of his boyhood's days, the large-hearted but politic Marquis of Posa. Fired with the desire of emancipating

Flanders from the Spanish yoke, the Marquis, conceiving that his design might be thus promoted, encourages Don Carlos's hopes in the terrible circumstances of which in confidence he had been acquainted by him. He tells Carlos he has come to him not as a former playmate, but sent to him by a "heroic people much oppressed."

"The Flemish Provinces
Weep on your neck; and with solemnity
Storm at your heart for their deliverance.
Lost is your country loved if Alva should—
That hangman's servant of Fanaticism—
March on to Brussels with the Spanish laws.
On the Emperor Charles' glorious grandson rests
Of these most noble countries the last hope;
That hope's extinguish'd if his noble heart
Has for humanity forgot to beat."

He offers his services to gain an audience with the Queen, which is, as might have been expected, grasped at by the misguided prince.

"*Marquis.* The King—so it is said—will to the city back
Without delay return. The time is short.
If you would with the Queen in secret speak,
It only can in Aranjuez be.
The quiet of the place—of country life
The customs unconstrain'd—favour—
"*Carlos.* Ah me!
I hoped so too—'twas vain.
"*Marquis.* Oh, not quite so.
At once to her myself I will present—
If she—in Spain—be still the same as what
She was in former times at Henry's Court—
I'll find her open-hearted."

The Queen is portrayed by Schiller with all gentle and womanly graces, yet not without that noble bearing and firmness of purpose which enables her not only to pass scathless through the perils that beset her, but even effectually to command the love of Don Carlos to be bestowed on another object.

"*Queen.* O my dear Carlos!
O pitiable thou! for thee I feel—
Most deeply feel the unutterable pain
Which in thy bosom rages. Without end
Thy woe is as thy love—so without end
Would be the glory it to overcome.
Young hero! struggle 'gainst it, for the prize
Is worthy of a champion high and strong—
Worthy that youth through whose heart virtue flows
Bequeath'd by many royal ancestors.
Then man yourself, my noble Prince!—afresh
Of Charles the Great the grandson then begins
To struggle onwards, where dishearten'd end
Other men's children.
"*Carlos.* 'Tis too late—too late!
O God! it is too late!
"*Queen.* To be a man?
How great, my Carlos, then becomes our virtue
When our hearts break in exercising it!

> In high position Providence you placed—
> Higher than millions of your brother men.
> She, partial to her favourite, to him gave
> What she from others took; and millions ask:
> What merit had he in his mother's womb
> That he 'fore other men should valued be?
> Then do you vindicate Heaven's preference—
> Show your desert to march in the world's front,
> And sacrifice what none e'er sacrificed."

With glowing vehemence the translator makes her repel the declarations of Carlos, who, partly by mistake, partly by stratagem, has been admitted to her presence—a *faux pas* which, through the jealousy of her rival, the Princess of Eboli, who loves but is not beloved by Carlos, leads to their secret amour being betrayed to the King. As might be expected, his gloomy nature is roused to all the deep hatred of which he is capable. The gallant Posa, knowing the dangers of his friend and the ominous mood of the King—a husband wanting love's majesty, still one who doats yet doubts—suspects yet strongly loves—resolves to take upon himself the crime and accept the fate of Carlos. He is shot down in the presence of him he had resolved to save, while the King, with a show of relenting paternal love, would take his son back once more to favour and affection. This is spurned by Carlos, and the whole court listen appalled to his scorn as he addresses the murderer:—

> "O ye who stand assembled here, with fear
> And wonder dumb, condemn not ye the youth
> Who to his King and Sire applies such words.
> Look here! For me he died! If ye have tears—
> If in your veins flows blood—not molten brass—
> Look on this corse, and then condemn me not.
> (*He turns to the King with more composure and calmness.*)
> Perchance with expectation you await
> The ending of this tale unnatural?
> Here is my sword—you are my King again.
> Think you I fear your vengeance? Murder me,
> As you have murder'd this most noble man.
> My life is forfeited—that I know well.
> What now is life to me? Here I renounce
> All that I might expect upon this earth.
> Seek you 'mong strangers for a son—There lie
> My Kingdoms.
> (*He sinks down on the corse and takes no part in what follows. Meanwhile is heard in the distance a confused noise of voices, and the pressing on of a crowd. Around the King there is deep silence. His eyes traverse the whole circle, but no man meets his gaze.*)
> "*King.* What! Will no one give response?
> Each looks upon the ground—each countenance hid!
> My doom is spoken. In these silent looks
> I read it plain. My subjects have me judged."

Carlos resolves on flight to the Netherlands. A last secret but innocent leave-taking occurs with her who not long since was his betrothed. Here he is surprised by the King, and with this comes the *dénouement.* The dreadful services of the Grand Inquisitor have been held in readiness, and into his inexorable hands fall the ill-fated couple as the curtain descends.

> "This be my last deceit.
> (*He is about to seize his mask. The King steps between them.*)
> "*King.* It is thy last!
> (*The Queen falls down senseless—Carlos rushes to her and catches her in his arms.*)
> "*Carlos.* Heaven and earth! Is she dead?
> *King (coldly and quietly to the Cardinal Inquisitor.)*
> Cardinal! I've done my part—Now do you yours.
> [*The curtain falls.*"

Such is a necessarily brief outline of the plot of Schiller's drama as ably rendered in Wood's translation of Don Carlos; a translation which not only does credit to the author, but adds an additional lustre to that faculty which he so ably and with such versatility adorns. For in his rendering of the "Satires of Horace"—of the "Epistles and Art of Poetry" by the same classic author; and last, not least, in the work before us, there is to be found, in the words of a gifted critic, "an unconstrained and even triumphant flow of versification which murmurs poetry."

The Simplicity of Life: An Introductory Chapter to Pathology (pp. 118), *including an Appendix.* By RALPH RICHARDSON, M.A., M.D., Fellow of the Royal College of Physicians of Edinburgh.

> Whoever thinks a faultless work to see,
> Thinks what ne'er was, nor is, nor e'er shall be.
> Expect not such, my friend, from me.

London: H. K. Lewis: 1873.

THIS is an inviting small quarto, neatly printed on good paper, and elegantly bound. We have read it with care, and a desire to do it every justice; but it took us some time to obtain a glimpse of the author's aim, and that glimpse was unsatisfying. He quotes, in an early note, some sensible observations by Dr L. Beale and the late Dr John Fletcher, an eminent teacher in the Edinburgh Extra-mural School, on the extreme importance of clearness of definition, and of accuracy and uniformity in the use of terms, in all philosophical inquiries. Having, we confess, a great belief and confidence in such modes of procedure, we entered on the task of perusal with expectation and hopefulness. But, alas! our author teaches more by precept than by example, and does not attain, we fear, to his own *beau idéal* in scientific disquisition. What he means by the "simplicity of life" we have failed to discover; for, after many references to previous writers, and a considerable display of learning—the greater part of it, however, gathered from a few years before and after 1830—we are led up to the following conclusion, which appears to leave the subject of life, instead of simpler, a little more complicated than before:—"The notion of life to be deduced from what has preceded is, that it consists in the sum of the characteristic actions of organized beings, performed in virtue of a specific susceptibility acted on by specific stimuli; and as this susceptibility and these stimuli, when natural, may be regarded respectively as the *predisposing* and *exciting* causes, as it were, and the actions resulting from them as the *proximate* cause of health, so it is of some change in the first that every predisposing cause, of some change in the second that every exciting cause, and of some change in the last, that every proximate cause severally consist."

We suppose that, when all the susceptibilities cease to be acted on by the stimuli, life is at an end and death occurs. But are we any the wiser? It humbly seems to us that the title of the book is not in keeping with the contents, and that our author has added nothing to our knowledge of a subject which has baffled all the great thinkers of the world, from Plato downwards. The nineteenth century is at a disadvantage for such speculations. It greatly lacks the leisure, the quietness, the patience, and, we may add, the reverence which these high mysteries demand. We would therefore advise Dr Richardson, whose lot has been cast in an age of bustle and competition and material prosperity, to accept his position, and, in future, to apply his mind, which is evidently vigorous and well furnished, to topics of a more practical and useful nature.

Hygiene Publique. La Cremation des Morts en Italie. Par le Docteur PROSPER DE PIETRA SANTA. Pamphlet, pp. 19. Paris: J. B. Baillière et Fils: 1873.

IN this most learned, well-reasoned, and clever pamphlet, the author, in the form of a letter to the editor of the *Union Medicale*, discusses the question of the proper means of disposing of the bodies of the dead. This subject has excited considerable attention in Italy; the well-known names of Coletti, Castiglioni, Polli, Gorini, and Brunetti, and many others, having written and spoken about it during the last few years. The Royal Society of Science and Literature of Lombardy have offered a prize, to be given in 1877, "for the best method of burning bodies as a substitute for burial, to show, by experiment on animals, that this method is free from inconveniences, that it is speedy, economical, not contrary to use and wont, or a violation of propriety."

This pamphlet goes fully into the subject,—describing, first, the necessity for the body of the dead returning into its constituent elements; the importance, for the fertility of the earth, that these should not be lost; *the impossibility of making mummies of all the race* —for had this been done with all for even three thousand years, by this time the dead would have displaced the living, nor would there have been a corner of the earth not filled by a mummy. Temporary and permanent cemeteries each have their own difficulties and inconveniences. It is proved from history that incremation is old, and urn-burial was considered most honourable. Greek, Hebrew, Roman, and Hindoo practices are all quoted. The experiments of Polli and Brunetti, of late years, are then detailed.

Sentimental, religious, and scientific objections are fairly stated and well met; among the latter we find the somewhat curious argument, If burning of bodies had always been the fashion, what would the sciences of anthropology and phrenology do? The most formidable objection of all is that furnished by legal medicine—exhumations would be impossible, and justice might be defeated. So far true: wounds might not be recognised, but mineral poisons might be detected. Still, *Salus populi suprema lex esto.* The last objections are the inconvenience of the cinerary urns when they accumulate in numbers, and the expense of incremation. These are mere matters of detail easily got over, especially the latter, as by burning several bodies at a time, and especially by the use of gas, the expense of each would not exceed five shillings—a most moderate sum, as the coffin is not needed, nor cemetery space. The *gravest* objection of all in our eyes is not alluded to. If every one wants to be burned, where are we to get subjects for dissection? unless indeed the burning, like the burials, in such cases be postponed.

Our author does not wish to make incremation imperative; he would leave burials optional for those who did not fancy incremation for themselves or their relations. Indeed, by his moderation of tone and suggestive reasoning, he anticipates Sir Henry Thompson in his recent article in the *Contemporary Review.* At the Vienna Exhibition Sir Henry had been struck with the results of the incineration of bodies and parts of bodies by Brunetti, who is Professor of Pathological Anatomy at Padua. The ashes of a body weighed about three pounds and three-quarters. "They were of a delicate white, and were contained in a glass box about twelve inches long, by eight inches wide, and eight deep." The fuel was wood, and cost only two shillings and fourpence.

The subject is not new to us in the northern metropolis. The late Sir J. Y. Simpson often proposed the substitution of burning for interment, and pointed to the unusual facilities the inhabitants of Edinburgh enjoyed for having the process efficiently carried on in their immediate neighbourhood. The lonely valley of the Hunter's Bog was regarded by Sir James as another Valley of Hinnom, where funeral fires could be kept perpetually burning; and no one discussed the subject in all its bearings more powerfully, or regarded with greater aversion the funeral customs of the present day. We agree with Sir Henry Thompson, that the practice he recommends just needs a beginning to ensure its gradual adoption; but he seems to be unaware of the existence of a club in Hamburg, numbering over eighty members, each of whom on entering makes a will, according to which his remains are to be burned. This club was only heard of last November, and no accounts have reached us of the incineration of any of the members. But we live in hope, and we are fully convinced that a single well-authenticated case of burning, recognised by law and tolerated by the community, will inaugurate a most important sanitary revolution.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING III.

Wednesday, 7th January.—Dr P. D. HANDYSIDE, *retiring President, and afterwards* Dr RUTHERFORD HALDANE, *President elect, in the Chair.*

Exhibition of PATHOLOGICAL SPECIMENS, etc:—

I. *Dr George W. Balfour* exhibited a CAST of the HAND of a patient labouring under congenital cardiac disease accompanied by cyanosis, in which bulbousness of the finger-joints and aduncation of the nails were specially well marked. The cardiac affection was complicated as is usual in such cases, its most important element being evidently constriction of the pulmonary artery.

II. *Dr Matthews Duncan* exhibited an INSTRUMENT, the invention of Professor Spiegelberg of Breslau, for performing operations by the GALVANO-CAUTERY. He regards it as a great improvement on that of Middledorpff, but thinks it might still further be improved by splitting of the handle into two separate halves, so that it might be used to remove tumours by shaving rather than cutting. The galvano-cautery is not only a most useful instrument, but also illustrates a curious subject in the history of medicine, when we remember the tirades against the cautery as the most horrible and cruel of all medical agents. It now really turns out to be one of the least cruel and least painful. Dr Duncan has used this instrument in two cases of severe malignant disease of the vulva, with much benefit; the operations could not have been performed by the knife on account of the hæmorrhage, and by this instrument they were done in two or three minutes, without the loss of a drop of blood, and with wonderfully little pain or scalding after the operation—much less than after a cutting operation. Dr Duncan has no doubt that in time the use of this instrument will be much extended.

III. *Dr Watson* then showed a SPLEEN weighing nearly 12 lbs., which, on the 1st November 1873, he had removed by gastrotomy from a man who had noticed the tumour for more than two years. It filled a great part of the abdomen, displacing the organs. The patient was reduced to a most anæmic condition, and pressed the performance of the operation. The pedicle of the tumour was transfixed by a strong double ligature and tied in two halves. The vasa brevia from the stomach gave much trouble in securing them.

IV. *Dr Watson* showed the base of the LOWER JAW, and the soft parts forming the floor of the mouth, as well as the muscular structures constituting the base and body of the tongue, removed on account of epithelioma.

V. *Dr Watson* showed the base of the LOWER JAW, and the soft parts forming the floor of the mouth, removed on account of epithelioma originating in the frenum.

VI. *Dr Watson* showed an OSTEOMA of the angle of the LOWER JAW, involving the posterior part of the base and the ascending ramus as high as the root of the coronoid process. The operation was performed sub-periosteally without opening the mouth, as the molar tooth, which had cut the gum, remained attached to the mucous membrane. The bone had been completely reproduced.

VII. A TUMOUR removed from the pterygomaxillary fossa, which projected into the pharyngeal cavity and pushed the parotid gland outwards beyond the angle of the jaw. The operation was practised in the same manner as in the case operated upon some years ago by him, and published in the *Lancet*, which was the first case in which such an operation had been practised or proposed for the removal of such tumours. The tumour was fibro-nucleated in its character. Dr Watson stated that the case in which he had formerly operated was quite well. In it the late Mr Syme had refused to interfere, and desired the patient to permit no one to meddle with it.

VIII. *Dr Watson* showed a PENIS which he had removed from an elderly patient on account of epithelioma commencing in the frenum. The example was interesting, as bearing upon the influence of congenital phimosis in the production of such disease. Here the phimosis followed upon the thickening of the prepuce, and did not precede the devolopment of the disease. Dr Watson amputated the organ in a somewhat novel manner, which he introduced into operative practice some years since. This procedure consists in amputating by direct section, then dissecting up the corpus spongiosum urethræ and turning it out through a slit in the cutaneous tissues clothing the under surface of the limb of the penis. The bleeding was staunched by transfixing the corpora cavernosa by a double catgut ligature, which being divided, each end was tied so as to include each half.

IX. *Dr Watson* showed a CALCULUS, removed from a boy æt. 8, coated with phosphates, but consisting chiefly of oxalate of lime, in which he had operated recently.

X. *Dr Watson* showed a CALCULUS, removed from a boy æt. 10, consisting of a phosphatic surface, but uric acid nucleus.

The retiring President, *Dr Handyside*, then delivered a most interesting valedictory address, which will appear in an early number of this Journal *in extenso*. After referring to his presidency including in it the jubilee year of the Society, he gave an account of the foundation of the Society in 1821, alluding to the very high literary and scientific eminence of Edinburgh society at that time, there being no fewer than eighteen scientific and literary societies in existence then, four additional to which were founded that same year. He told of the fifty-three original members, three of whom still survive. He then gave a most elaborate and interesting *résumé* and estimate of the early papers read to the Society under various heads, embracing the different subjects in the science of medicine and its collateral or subsidiary branches; also papers on statistical, philosophical, and literary subjects. He showed how the members of the Medico-Chirurgical Society had ever been in the van, and claimed, as the motto or watchword of the Society, "The indestructibility of Progress." He then traced the chequered career and migratory habits of the Society, as indicated by the *eighteen* different habitations during the first forty years of its existence, before it found the more settled abode it now has occupied for thirteen years. After alluding to the excellent tone which had characterized all the debates during his presidency, and thanking the members for their kindness, Dr Handyside then said farewell, and introduced Dr Rutherford Haldane to the Society.

Dr Rutherford Haldane thanked the Society for his election, and hoped that the meetings during his presidency would be as successful as they had been in the past.

Dr Matthews Duncan, as one of the oldest members present, rose to move a vote of thanks to Dr Handyside for his most interesting, important, and valuable address, which was entered into with a true philosophic spirit, and should be a guiding-star to the Society in its future labours. He moved that Dr Handyside be requested to allow his address to be published at the expense of the Society.

Professor Spence briefly seconded the motion.

Dr Handyside thanked the Society.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING II.

26th November 1873.—Dr THOMSON, *President*, in the Chair.

I. Adjourned discussion on Dr Milne's paper. This paper will be found at p. 707.

Dr Keiller thought that a very great deal might be said in favour of all the methods of treatment mentioned; but that, of course, before deciding on any line of practice, we should carefully consider the history of the case, the stage of the labour, and the present condition of the patient. He would not at present discuss the subject, as he was absent when Dr Milne read his paper; but he perceived that Dr Duncan had referred to a case, in which he stated that he had delivered a woman at the seventh month, without much difficulty, of a dead child, through a pelvis a little more than three inches in the conjugate diameter. If that case referred to a woman who had been recently in the Maternity Hospital, he did not think that Dr Duncan had stated the patient's history fully enough, as he did not mention that he (Dr Keiller) had, on one occasion, delivered her of a live child at the seventh month. As the case had been recorded by him in the first volume of the Society's Transactions, he would read it without any further remarks. He afterwards showed a pelvimeter, which he had received from Dr Basset of Birmingham, and which he considered one of the best instruments of the kind he had ever seen.

II. NOTES OF A CASE OF CEPHALOTRIPSY, by *Dr Brotherston*, with photographs showing the head, with the instrument applied. This paper appears in the present Number, p. 715.

Dr Keiller recommended that, in such photographs, the preparation should always be suspended, as a dark background was apt to make it indistinct.

Dr Simpson thought the Society was indebted to Dr Brotherston for bringing this before them, especially as some of the Fellows at last meeting seemed to doubt the occurrence of such a degree of pelvic deformity in Edinburgh or the neighbourhood.

SESSION XXXIII.—MEETING III.

Wednesday, 10th December 1873.

Dr J. Matthews Duncan read an INAUGURAL ADDRESS, the subject being, "The Extension and Increase of the Scientific Spirit in Medicine" (which appears at page 673); and a cordial vote of thanks, on the motion of Professor Sanders, was awarded to Dr Duncan for his able paper.

SESSION XXXIII.—MEETING IV.

Wednesday, 24th December 1873.—Dr J. MATTHEWS DUNCAN in the Chair.

NOTES OF PLACENTA PRÆVIA. BY DR M'DOUGALL, COLDINGHAM. COMMUNICATED BY PROFESSOR SIMPSON.

CASE 1.—Mrs R., aged 40. Thin, spare woman, mother of six children, and has always been easy in her confinements. On the 24th of January I was summoned to attend her immediately, as she had been "seized with a severe flooding." I accompanied the messenger, and, on arriving at the house, found the woman in a condition of very great exhaustion, and blood dropping on to the floor. Making a vaginal examination at once, before asking any questions, I found the os only dilated to about the size of a sixpence, rigid, and undilatable, with a soft substance wholly covering its surface. With some difficulty I introduced my index finger, and discovered the placenta to be firmly adherent all round, a very trifling separation having taken place at the posterior part. As it was impossible to deliver with the os in such a rigid and undilated state, I decided upon plugging the vagina, with the double object in view of allowing time for the os to dilate, and the woman to recover somewhat from the utter prostration in which she was. This I accordingly did by completely filling the vagina with cotton-wool; thereafter, I administered an ounce and a half of brandy in milk, and had the satisfaction to find, in the course of a quarter of an hour, that the patient rallied, and was able to give an account of her illness. She was between seven and eight months pregnant, and expected her confinement about the middle of March. Two weeks ago, while engaged with a washing, and exerting herself more than usual, she was alarmed by a sudden and profuse flow of blood, which caused her to faint. She was confined to bed for four days, a slight sanguineous discharge continuing during that time, with no pain. She rose on the fifth day, and was able to attend to her household duties; and although she continued to lose a little blood occasionally, no flooding of any consequence took place until the 24th of January, when I was sent for. Naturally of a ruddy complexion, she was blanched, with small fluttering pulse, sighing breathing, and semi-comatose.

After four hours I removed the vaginal plug, and ascertained that very little bleeding had taken place in the interval, a small portion only of the wool being stained. An immense gush followed the removal of the portion in contact with the os. The os was now dilated to about the size of a half-crown, and the placenta at the back part pushed down a little. As it was impossible, without inflicting excessive pain, and using great violence, to introduce the hand into the uterus, with my two fingers I effected as complete a separation as possible. A fearful loss of blood took place while I was engaged in detaching the placenta, but immediately I felt it loose in the cavity of the uterus, all hæmorrhage ceased. With my hand still in the vagina, I administered a large dose of ergot in two ounces of brandy and some warm milk, and waited for half an hour. No pain followed. I therefore ruptured the membranes, which protruded somewhat through the os. The patient rallied wonderfully; but there were still no pains. As the os was now, however, sufficiently dilated to admit the hand, and as I was hurried, I decided upon delivering the woman by version, rather than leaving the case to be completed by nature. I seized on both feet, and met with no difficulty in extracting the child, till the head reached the brim, when considerable force was required to draw it through. There was no further loss of blood, and the mother made an excellent recovery.

CASE 2.—Mrs P., aged 28. Strong, robust, plethoric woman, about eight months pregnant with her first child. For ten days a sanguineous discharge has continued, with only short intermissions, but without pain. She did not, however, seek for medical advice till alarmed by striking anæmia, and a feeling of progressive exhaustion.

Present Condition.—Countenance pale, lips and gums bloodless, eyes sunken, with dark areola round them, conjunctiva dull, but not insensible. She was in a lethargic condition, but could be roused by loud talking. Skin perceptibly cold. The thermometer indicated a temperature of scarcely 97°; pulse extremely feeble, very irregular, and so indistinct that it could not be counted; great restlessness, irregular breathing, and other symptoms, indicating the most profound asthenia. These symptoms had come on very suddenly, having been preceded by an immense loss of blood, accompanied by violent uterine contractions. Her condition of exhaustion was so great at the time of my visit, that I considered her case quite hopeless. A vaginal examination revealed a state of matters extremely discouraging. The os could scarcely be reached, was soft, considerably swollen, and not dilated beyond the size of a goose-quill; about three inches of a soft substance, as large as an ordinary earthworm, protruded from it, which I looked upon at first as clotted blood, but finding it resisted my attempts to remove it, I concluded to be a prolongation of the placenta. My further knowledge of the case confirmed this opinion. The case was an extremely embarrassing one; blood continued to ooze constantly from the vagina, and at intervals enormous gushes took place, bringing the patient to the very verge of death. Continuing to administer stimulants and fluid nourishment in large quantities, I applied a piece of lint dipped in the perchloride of iron to the os, and thoroughly plugged the vagina. Being fully satisfied that the bleeding was quite checked, and the pains continuing feeble, I left the house, and did not return for *twelve hours*, when I was sent for, as the "flooding had returned worse than ever." On arriving, and finding my patient more exhausted than she was when I left her the night before, I removed the silk handkerchiefs which I had used in plugging the vagina, and found them to be thoroughly soaked with blood. I was still further disappointed to find no progress made in the dilatation of the os, and fluid and clotted blood continued to escape from her in large quantity. Finding it impossible to dilate the os with the fingers, I introduced a catheter and ruptured the membranes posteriorly. This had the effect of increasing the pains in frequency and strength. Each pain was followed by a considerable quantity of coloured fluid, which, however, I was happy to observe did not increase her exhaustion.

After waiting a reasonable time without any advance taking place, I administered chloroform, and had the satisfaction of finding that, when completely anæsthetized, her pulse became fuller and more regular. Whilst thoroughly under the influence of chloroform, I tried to dilate the os; but after an hour's persevering effort I was only enabled to introduce my index finger. A firm, narrow band was felt to pass round the whole inside circumference, and obstinately resisted all attempts at further dilatation. It was now evident that, unless something was done immediately to check the fearful hæmorrhages which every now and again took place, the woman must very shortly succumb to the repeated and excessive losses of blood. As the finger could not gain an entrance, I passed the uterine sound, and carried it deliberately round and round until a complete separation was effected. After this I was greatly relieved and delighted to find the flooding completely checked; and although delivery did not take place for four hours afterwards, the further loss of blood was very trifling. After the detachment of the placenta, the pains set in very actively, but the os still resisted all efforts to dilate it. Seeing it to be quite hopeless to overcome the stricture by digital manipulation, I notched it with a probe-pointed bistoury at four different points, and I was immediately thereafter able to introduce three fingers, with which I seized upon the placenta, and dragged the whole mass into the vagina, which was shortly thereafter extruded. The passage of the placenta assisted to dilate the os still further. The head now presented, and was slowly advancing. Being anxious to husband the woman's strength (already sufficiently low), I applied the long forceps and completed the delivery. This patient made a satisfactory but an exceedingly tardy recovery. The stricture of the os was a very troublesome complication, and rendered the management of the case peculiarly difficult. I learned some time afterwards that a few years ago she had been treated for ulceration of the os by escharotics, and it is very likely that the stricture dated from that period. Such an obstinately rigid condition of the os in placental presentations must be very uncommon, for it is generally stated that in such cases the passages are soft and easily dilated. The course which I adopted in this case has not, so far as I am aware, been before followed. The loss of blood which followed the notching of the os was considerable, but by no means alarming. I have frequently wondered since how this woman rallied from the tremendous hæmorrhages.

CASE 3.—Mrs P., aged 30. Slender, scrofulous woman, eight months pregnant with her second child. Ten months ago she was delivered of a still-born male child. From those in attendance on that occasion I learned that the labour was an exceedingly easy one, and that the afterbirth came away without any greater loss of blood than usual. She made a good recovery, and required no medical attendance.

In the month of March, when she considered herself eight months pregnant, she consulted me, complaining of a dull, constant, fixed pain in the right lumbar region, feeling of nausea, and obstinate vomiting. She had a languid expression of countenance, and complained of a feeling of coldness over the abdomen. The pulse was slow, and wanting in fulness. Tongue broad, flabby, pale, and indented with the teeth; little appetite; gnawing sensation at epigastric region, with a constant feeling of sinking. For the last three or four days the fœtal movements had been more vigorous, but now they are much weaker. The placental murmur is very distinct, but the cardiac very faint, scarcely audible. I diagnosed placental disease, and advised the induction of premature labour, with the view of saving the child. The mother would not, however, consent to this, and I therefore merely addressed myself to the relief of symptoms. I applied six leeches to the seat of pain, gave oxalate of cerium to check the vomiting, and chlorate of potash as an oxygenating agent. These measures had the effect of relieving the more distressing symptoms. Four weeks after, I was sent for to her confinement, when I discovered unmistakable evidence of the death of the fœtus, viz., the abdomen was flaccid, and wanting in that tonicity usual at the full time; no fœtal motions had been felt for some time; a fœtid discharge had taken place from the vagina for upwards of ten days; no uterine or cardiac sounds could be heard. Labour began ten hours before I was sent for. The pains, however, continued very feeble, and very little progress had been made by the time I arrived at the house. The os was the size of a crown-piece, soft and dilatable. The placenta, lying loose in the cavity of the uterus, covered the os; the finger could be introduced at any part and carried round and round without meeting with any interruption. With little difficulty I dragged the whole mass into the vagina, and found it to be preternaturally enlarged, very irregular in form, more distinctly lobulated than usual, several apoplectic clots scattered over its surface, and its entire substance was affected with fatty degeneration. A portion examined under the microscope showed it to be entirely composed of large oil globules. The placenta measured 32 inches in circumference, 10 inches in diameter; thickness varied at different parts from 1½ to 3 inches. There was some difficulty in ascertaining the nature of the presenting part, in consequence of the great thickening of the scalp from serous effusion into the cellular tissue. The pains being very feeble, I applied the forceps and delivered the head, but was unable to complete the labour till the abdomen was emptied of its fluid contents. On examining the child shortly after birth, I found the cuticle loose at many parts, and easily removed. The entire surface of the body was much thickened from serous effusion into the subcutaneous areolar tissue; a sero-sanguineolent fluid oozed from incisions made into it. An examination of the internal parts revealed nothing abnormal. The mother, naturally of a weakly constitution, made a very tardy recovery, and is only yet recovering from sub-involution of the uterus.

CASE 4.—Agnes L., aged 28. Was sent for to this woman, having attended on two previous confinements, forceps being required on two occasions in consequence of the size of the child. On making a vaginal examination I found the umbilical cord well down, and quite pulseless; the os fully dilated; and the placenta, lying with its fœtal surface over it, could be moved in any direction, not having any connexion with the uterus. A very considerable loss of blood took place before I arrived. I removed the placenta at once, and a few pains expelled the child shortly thereafter, which was strong and well developed, and evidently had only been dead for a very short time.

A case exactly similar to the above is recorded in the 1st vol. of Sir J. Y. Simpson's Obstetrical Works, regarding which he stated that, so far as he was aware, it was *unique.*

Dr Duncan said he would not enter on the subject-matter of the paper, but would put a single question, the right answering of which was of the utmost importance in a truly scientific point of view. The question was, What degree of expansion of os was requisite in order to allow the extraction of the placenta without injury to the os in these cases? There had as yet been no measurements, and these were indispensable to a proper settlement of, or judgment as to, practice. We had, perhaps, improved details, but no new law had been achieved since the time of Levret. He was grateful to Dr M'Dougall for reporting the cases.

Dr Macdonald said that Dr M'Dougall, in his interesting collection of cases and remarks thereon, had thrown no new light, for example, on the question as to what caused the arrestment of the flooding after separation of the placenta. In reference to treatment, he had ignored the dilating bags which he and others had found so useful. Dr Duncan's question appeared to him of great importance. If the placenta could be removed, he rather thought that the os would be expanded sufficiently for the hand to pass in, and version be accomplished. Dr Macdonald would not attempt complete separation; and he was of opinion that the evidence against complete separation more and more accumulated. Dr M'Dougall's fourth case was singular in this respect, that there had been separation without uterine contraction. In regard to fatty degeneration, that was a wide term, embracing as it did more than the mere deposit of fat in cells. There were often extravasations without disease; and he had lately examined a placenta attacked with colloid degeneration, where there was no syphilitic history.

Professor Simpson, referring to Dr Duncan's question, said he would not hazard a guess, but would be surprised if a placenta could not pass through a less expanded os than the child.

Part Fourth.

PERISCOPE.

INFLAMMATION OF GANGLION CELLS.—The publication by Cohnheim of another contribution towards the nature of the inflammatory process, in which he again brings prominently forward his belief that pus-cells originate exclusively in the transudation of white blood-corpuscles, gives special importance to some researches lately made by Dr Andrew R. Robinson of New York. A short summary of his conclusions is published in the last number of the *Wien. Med. Jahrbücher*, which is rendered very intelligible by the drawings which illustrate the paper.

Dr Robinson's method consisted in passing a thread through the sheaths of the aorta of the frog, and in then sewing up the abdominal wound. The frog was killed in from two to seven days afterwards. The aorta was then treated *lege artis* by chloride of gold, and the ganglion cells and nerves isolated by teasing.

The appearances found in the cells varied in degree according to the length of time the animal had been allowed to live, and, briefly summarized, were the following:—The protoplasma of the cell lost its usual coarsely granular aspect, the capsule showed in whole or in part groovings, the effect of which was to present an appearance to which the term mosaic might be applied, and the contents of the cell consisted to a greater or less extent of bodies having all the objective characters of pus-cells. Sometimes the cell was completely filled with these pus-corpuscles, and in these cases they extended beyond the cell into the space corresponding to the axis-cylinder of the nerve. At other times, in one part of the cell the process of division was advanced, whilst a line of demarcation separated it from the other part in which it had not yet begun. In young ganglion cells, in which the capsule offers little resistance, the newly-formed cells escaped early through the capsule; in older cells with a thicker capsule, they remained enclosed for some time; but, in all cases, they eventually escaped into the surrounding tissue.

Having in view the doctrines generally held in regard to division of cells, Dr Robinson lays especial stress on observations of cases in which the protoplasma could be seen to be in process of division, independently of the nucleus. He found that division of the nucleus did not necessarily precede division of the protoplasma of the body of the cell, from which and from the axis-cylinder pus-cells can arise independently, and is of opinion that the observed division takes place, not because of any influence that the nucleus has on the protoplasm, but because they are both still in the condition of living matter. T.

M. DEPAUL related to the Academy of Surgery the case of a woman on whom he had performed the operation of gastrotomy for extra-uterine pregnancy. The difficulty in diagnosis was very great, and did honour to the learned professor. After the laborious extraction of the fœtus, he attempted to remove the placenta, but was prevented by numerous and deep adhesions to all the abdominal organs, so gave up the attempt, and tried rather to prevent the risks of putrid infection to which the patient was exposed. For six days all went well, but on the ensuing night the patient died in a few seconds of hæmorrhage into the abdomen. M. Boinet, on the same occasion, recorded a similar case, with like fatal result, from the same cause.

Without disguising the enormous difficulty of extracting the placenta in such cases, these are not worse than are met with and overcome by coolness and patience in many cases of ovarian cysts with thin walls, and in extensive vascular adhesions; we think that the dangers of primary hæmorrhage are at least counterbalanced by the risk of sudden fatal secondary hæmorrhage, and the not less certain result of purulent infection. In a word, an almost certain death as the result of a placenta left in the abdomen ought to warrant any possible effort for its removal.—*Dr F. de Ranse, in Gazette Médicale de Paris*, No. 52, 1873.

POLYPUS OF THE LARYNX—REMOVAL BY SECTION OF THE THYROID CARTILAGE—RECOVERY.—Dr Laroyenne of Lyons reports the following case:—Female, aged 33, emaciated by suffering; has had periodic attacks of difficulty of breathing every eight or ten days for two months. The voice is barely audible. A laryngoscopic examination shows a tumour which nearly closes the glottis, passing into the rima during inspiration, and being forced out again in expiration. After various unsuccessful attempts to remove the tumour from the mouth, it was resolved to open the thyroid. This was done by a curved transverse incision with its concavity upwards, and its lowest part midway between the two edges of the thyroid cartilage. A complete vertical section of the cartilage was then made after it was exposed. An attempt was made to remove the tumour, but hæmorrhage excited cough and concealed the parts; so, after some fruitless efforts, extraction was postponed till next day, when it was easily accomplished. The tumour was as large as a cherry, and was a sarcoma. The wound soon healed, and the patient's voice was not improved, being much the same as it was before the operation,—that is, very feeble, audible at a distance of eight to ten paces.—*Gazette Hebdomadaire*, 1873, No. 49.

STATISTICS OF BIRTHS AND DEATHS.—From the *Monde Russe* we extract the following:—In 1867, there were in Russia 2,239,652 deaths, or 35·8 per 1000. At first sight this appears very high, as Kolb has shown that in Italy the proportion is 32·7, in Prussia 26·8, in Belgium 23·7, in France 23·2, in England 22·5, in Sweden 21·4. But its true significance is seen when compared with the births, as seen in the following table:—

In 1000 inhabitants,

	Births.	Deaths.	Proportion of Births to Deaths.
Prussia, .	38·4	26·8	143·6 per cent.
Belgium, . .	32·9	23·7	142·4 ...
France, . .	26·3	23·2	113·6 ...
England, . .	35·1	22·5	156·0 ...
Sweden, . .	27·5	21·4	130·9 ...
Italy, . . .	37·1	32·7	115·8 ...
Russia, . .	49·0	35·8	138·5 ...

La France Médicale.

FRACTURE OF THE CLAVICLE TREATED BY PLACING THE ARM BEHIND THE BACK.—M. Broca recently treated a case of fracture of the left clavicle, in which the fracture was near the middle of the bone, was oblique from above downwards and from without inwards, the fragments riding one another considerably. After numerous plans of treatment had failed to reduce the fragments, M. Broca adopted a suggestion made last year by Dr Michel to the Société de Chir., and placed the arm in a semiflexed position behind the back, where it was retained by a bandage for eighteen days, with the effect of completely adjusting the fractured surfaces and producing an excellent cure. For a few days longer a sling was used. The patient, a man of considerable nerve, complained of the inconvenience and pain of the method for only the first twenty-four hours.—*Jour. de Méd.*

Part Fifth.

MEDICAL NEWS.

BLOODLESS SURGERY.—Under this somewhat sensational title, the journals of the day are full of a method of arresting hæmorrhage during operations, devised by Esmarch of Kiel. To empty the limb by bandaging with an elastic bandage, and to control hæmorrhage by an elastic belt or strap of indiarubber, are the salient points of the method. Without detracting from the originality of the eminent surgeon of Kiel, it is interesting to look back on the past, and see how much of the real value of this method was known to our ancestors. Thus Sir Charles Bell, in his "Illustrations of the Great Operations of Surgery," published in 1821, says:—"I may here observe that by the management of the tourniquet blood

may be gained. If the limb be uniformly rolled before amputation, the veins are emptied into the general system, and blood is saved instead of being withdrawn. In a very exhausted state of the patient it may be of service to attend to this."—P. 58. We believe that by posture and an ordinary roller to empty the limb of fluid blood, is safer than, by an elastic roller, to force out of the limb not blood only, but perhaps putrid discharges; and that an ordinary tourniquet, properly applied, with a circular bandage, and without a compress, exerts as much compression as is needful, and not enough to imperil the life of the flaps, as there is too much reason to fear, from some cases already recorded, the elastic band may do. The surgeons of the Edinburgh Infirmary have long practised the above simple means of emptying a limb of blood; and by it have performed operations for necrosis, for bone-abscess, and very prolonged dissections in excision of the wrist, elbow, and ankle joints, with as much ease as in the dead subject, as far as hæmorrhage is concerned.

THE BRITISH MEDICAL BENEVOLENT FUND.—The Committee of the British Medical Benevolent Fund earnestly appeal, at this season, to the profession for help towards the relief of the many cases of distress among their poorer brethren, which continue to reach the Committee from various parts of the kingdom. In addition to these urgent demands from the needy and destitute upon the department for immediate relief, the great desirability of increasing the slender amount to which the annuities are of necessity at present limited—and which ranges from £15 to £20—has especially pressed itself upon the consideration of the Committee. Indeed, a glance at the cost of the necessaries of life will be sufficient to show the painful inadequacy of such a pittance, which in not a few instances is all the recipients can count on for their own. In view of this a movement has been set on foot in the hope of obtaining a sufficient sum to enable the Committee, by investment, to raise the incomes of the annuitants (in succession, as the fund permits) to £26, or say ten shillings a week; and a nucleus has been already kindly contributed, amounting to something over £150. The details of cases within the ordinary experience of the Committee would amply support in the strongest terms this call for help. The Committee, however, feel confident that, even without such, the good cause they advocate will plead for itself to the generosity of the profession at large, and will not plead in vain. Contributions, either to "The Special Fund for Augmentation of Annuities," or for "the general purposes of the Charity," will be thankfully received by the treasurer, C. J. Hare, M.D., 57 Brook Street, Grosvenor Square; or by C. S. Webber, F.R.C.S., honorary financial secretary, 1 Upper Berkeley Street West, Hyde Park, W.; or by Joseph Bell, 20 Melville Street, Edinburgh, Hon. Local Secretary.

OBITUARY.

Francis Cornelius Webb, M.D., F.R.C.P.—We regret to have to record the death of Dr Webb, editor of the *Medical Times and Gazette*, which occurred, after a very short illness, on 24th December 1873. Born in 1826, he went through an apprenticeship to a general practitioner before he was seventeen, and then studied at University College, where, as a student, he took very high honours, afterwards graduating in Edinburgh in 1850. He settled in London and worked at general practice for nine years. He lectured on Medical Jurisprudence in the old Grosvenor Place School of Medicine, and afterwards on Natural History at the School of Dental Science. For many years he has been connected with the *Medical Times and Gazette*, first as sub-editor and afterwards as editor-in-chief. He had a very fully occupied and overstrained life—lecturer, editor, physician, man of letters—for he has left papers indicating great erudition and admirable powers of arrangement and description. He was almost certainly overworked, and symptoms of weak heart and angina pectoris proved most rapidly fatal. We owe much of the information in this brief notice to a most eloquent and feeling memoir in the *Medical Times and Gazette*, from the pen of one of the most industrious and original of scientific writers.

Charles Legros.—This most industrious and talented young physician has also fallen a victim to overwork, under bad hygienic conditions. An enthusiastic histologist, he has been working in an ill-arranged, ill-ventilated laboratory, and died in his thirty-sixth year from severe jaundice, which seemed to be the result of a chronic septicæmia. His name was already well known, not only in France, but in this country, as the author of many important and original observations, such as his memoir on Erectile Tissues, on the Origin of the Secretory Canals of the Liver, and especially, along with his friend Onimus, on Spontaneous Generation, and on Medical Electricity. He was recently teaching histology with great zeal as the assistant of Professor Robin.

Dr A. Watson Milne died at Lanowlee, Bombay, on the 13th January, of diphtheria, in the twenty-fourth year of his age. By many of our readers this sudden end to a young life of so much promise will be felt as a personal loss. Zealous and earnest in his professional studies, especially in all varieties of hospital work, he was most unselfish and obliging in his readiness to assist to the utmost of his power. Few men of his age had so many friends and such high prospects.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Valedictory Address to the Medico-Chirurgical Society of Edinburgh, delivered on the occasion of retiring from the Chair of the Society, 7th January* 1874; by P. D. HANDYSIDE, M.D., F.R.S.E., formerly Senior President of the Royal Medical Society, and Senior Acting Surgeon to the Royal Infirmary; Teacher of Anatomy in the Edinburgh School of Medicine.

IN the delivery of a valedictory address at the close of the Jubilee period of our history, during which I have had entire and unalloyed satisfaction in presiding at your meetings, I shall probably best discharge the duty devolving on me—though I feel keenly my incompetence to do justice to the theme—by taking a cursory glance at the origin, progress, and present state of the Medico-Chirurgical Society, in relation specially to the services it has rendered to Medical Science.

Although Scotland, as a distinct nation, possesses a history of its own, it has had the wisdom to follow other nations in many of its institutions for the advancement of science, such as the Royal Society—our throne of British science, the Royal Medico-Chirurgical Society, and the Royal Society of Arts. The year 1821 was, for Edinburgh, a remarkable epoch, she being then in the zenith of her fame as a school of Psychology, Philosophy, and Medicine, collateral with which there flourished in it no fewer than eighteen *scientific* debating societies.[1] Edinburgh, looked on at that date from another point of view, evoked the testimony of a French writer, M. Naudin, who had in that year visited us, to the effect that the many literary institutions which then existed here, taken in connexion with its politics and its architectural beauty, served to make Edinburgh, as he said, one of the most attractive of existing cities.

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[1] The Edinburgh Almanac for 1822.

It is to the credit of our two Royal Colleges that those of their members who were to form in 1821 the Medico-Chirurgical Society had previously furnished so many office-bearers to the scientific societies which then served to connect medicine with general science. Thus, among the members of the *Speculative Society* was a prominent medical man, Dr George Stedman; the *Royal Physical Society* had as president Mr James Russell, predecessor of Mr Syme in the Clinical Surgery chair; the *Physico-Chemical Society* had as president Mr Benjamin Bell; the *Royal Society* had, as councillors, Dr Andw. Duncan, jr., editor of the Edinburgh Medical and Surgical Journal, and Dr Thos. Chas. Hope; the *Royal Medical Society* had as a president Dr John Conolly, and as its treasurer Dr Andrew Fyfe; the *Caledonian Horticultural Society* had as its perpetual vice-presidents, Dr Andrew Duncan, sen., founder of our Harveian Society, Æsculapian Club and Royal Dispensary, and George Bell, surgeon to the King; and as a councillor, Dr John Yule, treasurer of the Royal College of Physicians; and the *Wernerian Natural History Society* had as president Mr Robert Jameson, Professor of Natural History, and as councillors, Dr James Gregory, jun., the late Sir Wm. Newbigging, and Dr Samuel Hibbert. At this fortuitous juncture were instituted in Edinburgh other four *scientific* societies. On the 21st April 1821 the *School of Arts* (now the Watt Institution and School of Arts) led the way; and this institution has now, "it is thought, a right to expect to be recognised as being in the van of Technical colleges." No fewer than 24 out of the 53 original members of the Medico-Chirurgical Society co-operated in the support of the School of Arts during the first year of its existence. In the same year was founded the *Royal Scottish Society of Arts*, and as its councillors we find the names of Drs Hope, Fyfe, Keith, Hooker, and Meikleham. Thirdly, the *Phrenological Society* was hopefully inaugurated, having as its vice-presidents Drs Richard Poole and Robert Hamilton, the latter the founder of the Medico-Chirurgical Society, and as its councillor Dr Sibbald, founder of the new medical bursaries. One good was effected by this society, now I believe languishing, inasmuch as it showed us how little we knew of the functions of the surface of the brain, though it led us, through the admirable instructions of its lecturer, Dr Spurzheim, in the year 1828, to adopt in this School his mode of dissecting the fibres of the spinal cord and the encephalon, *since* followed out by Solly, Foville of Rouen, and others. Lastly, the *Medico-Chirurgical Society* took its rise during this remarkable year. On 21st May, Mr William Brown, formerly secretary, and then vice-president, *one* of three of our original members happily now among us —for our Society's is an unusually heavy death-roll—published a pamphlet entitled "Remarks on the Expediency and Practicability of a Union of the Royal Colleges of Physicians and Surgeons in Edinburgh," and in a few weeks afterwards, Dr Henry Dewar, Fellow of the Royal College of Physicians, published a rejoinder, entitled "Observations on the present relative situations of the Royal Colleges of Physicians and Surgeons of Edinburgh," in which he urged that "the members of the Colleges are not yet ripe for a union. It is liberal," he continues, "to keep the object in view." It is impossible to read the harmonious utterances of these esteemed, then senior, Fellows of our Royal Colleges without regarding them as somewhat representative of a certain yearning for union between these two scientific heads of the medical profession in this city, and which union in scientific work was then being harmoniously accomplished by the institution of the Medico-Chirurgical Society.

At this date the study of *Animal Physics* had a great impetus given it from the labours of Chevalier (since Baron) Bunsen and Dr Neil Arnott. The exertions of Oersted, Sir H. Davy, Wollaston, Ampere, and Hansteen had brought to light many *Chemical Phenomena* connected with voltaic action and the intensity of the magnetic force; new vegetable alkalies were also being searched for by Brandes. In *Zoology and Comparative Anatomy* Dr Robert Grant—a *second* of our original members, happily now fulfilling his duties in University College, London—was then doing valuable scientific work. In *Anatomy* Dr James Sanders, a man, like his son, of high scientific character, and a teacher of the Practice of Medicine in this School, pursued his researches on the renal plexus and on the structure and development of the fœtal brain. In *Physiology*, Prévost and Dumas were working with the globules of the blood in different animals, obtaining by experiments many interesting results; and were transfusing serum, water, and blood into the vascular system. Dr Sanders, again, was pursuing his investigations on the arterial pulse. In *Pathology*, at this period of time, the writings of John Thomson on Inflammation,—of Sanders, who had for some years been directing the notice of the profession to the morbid affections of the Spinal Cord, and to the results of his post-mortem investigations of the structures involved,—subsequently the well-known researches of Abercrombie on the pathology of the spinal cord, of the heart, and of the alimentary organs,—and, lastly, those of Kellie of Leith, by his remarkable investigations on the Pathology of the Brain, and especially on the circulation within the cranium,—all gave a marked impulse to scientific medical work in Edinburgh. In *Botany* Dr Jackson Hooker held a deservedly high place, with telling effect in that department. In *Materia Medica and Therapeutics*, Dr Sanders's well-known and novel treatise upon Digitalis in Phthisis had attracted deserved commendation at home and abroad; and in the hands of Dubois, Esquirol, Laennec, and Pinel had been translated into French, and published at Paris and Antwerp. Further, under this head it may be added, that, in 1821, a curious report on the comparative nutritive properties of food had been drawn up by MM. Percy and Vanquelin, of the French Institute. In *Surgery*, Robert Liston—who, besides having had the honour of being the instructor and guide of Syme, was indeed father of Scottish scientific surgery, and fond memories of whom haunt the schools, hospitals, and practice of Edinburgh and London—proceeded, by upsetting and banishing all the useless paraphernalia of our art, in establishing it upon sound pathology and therapeutics.

Philosophic thinkers and practical workers, then representing medical science in Edinburgh, had sagacity to perceive elements of stability in the Society that they purposed to originate, and 53 requisitionists accordingly, in May 1821, expressed their views to the following effect:—"By subscribing our names to this paper we testify our approval of the objects and constitution of the Medico-Chirurgical Society of London, and our willingness to co-operate in the formation of a similar institution in Edinburgh."

The character of its founders immediately inspired the profession, here and elsewhere, with strong confidence and a spirit of cordial co-operation; whereupon, at short notice, the Medico-Chirurgical Society—*as we are*—presented a Minerva-like birth, springing at once into her full maturity. Here we owe it to the memory of our first secretary, subsequently our president, Dr Robert Hamilton—an estimable man, and a teacher of ophthalmic surgery in this School—to pay him honour as the requisitionist who undertook and accomplished the formation of this Society. It may be added that the *Nestor* of the three requisitionists, or original members, happily still among us, is Dr John Gairdner; once our treasurer, afterwards our president, and who contributed to the Society several papers of high merit, and of permanent value.

The aim of the Society, from the first, has been to place medicine on a *Scientific* basis; thus rescuing it from the *Empirical* character that had so long distinguished it as a profession, and which still, it is to be regretted, characterizes much of its practice. Our founders saw that it is only on a broad basis approaching to exact science, and therefore on safe induction, that our profession could rest, and that a superstructure entitled to the respect and confidence of its members, as well as of observing men of other professions, could in time be raised. Anxious, accordingly, to elevate the qualifications of those standing at the portals of the profession, they personally cultivated with assiduity the whole field of medical Education; adding, meanwhile, to the stores of medical literature in papers and works, many of which are of imperishable merit. They aimed at the convergence of all the collateral branches of medical science, seeing how harmoniously they stand connected to each other; and thus they sowed the seed of that spirit of mutual co-operation of which we reap the fruit, and which we gratefully recognise as a marked feature in this Society at the present day. The training of our youth in the medical profession so much beyond the old standard has conduced materially to the elevation of our profession.

In particular, the enforcement of a somewhat comprehensive preliminary Education, and the cultivation especially of the Observant faculty, has of late years tended towards a farther advance in scientific medicine.

We thus see, Gentlemen, that the founders of the Medico-Chirurgical Society were men to whom a philosophical conception gave the formula by which alone, as men of science, they could work; and this working formula consisted, as it were, of a series of rings placed within one another, touching each other mutually at every point, and embodying, from within outwards, Animal Physics, Chemistry, Zoology, Anatomy, Physiology, Pathology, Materia Medica, Therapeutics, Surgery, Medicine, Obstetrics, and Medical Jurisprudence. Our predecessors were, from the first, careful in observing facts and compiling statistics under these several heads; they appointed committees of their number to report on certain communications read by their members; and they produced occasionally philosophical and literary papers bearing closely on their functions as Scientific Practitioners of the Healing Art. By this procedure they have left us a record of their work, together with the mode of its execution; thus enabling us, animated by their zeal, so to labour, in the prospect of each successive meeting of this Society, as that we may bear *each his own share* of work in rearing a scientific superstructure upon the broad basis they have so wisely laid. Practically, the effect of this doubtless will be, that our noble profession, in all its departments, will hold eventually a higher place in general estimation.

So far as your time will permit, I shall notice, under the twelve classified Heads already given, some of the scientific work that has been achieved by our Society.

I. *Animal Physics.*

In ANIMAL PHYSICS, "that bracing study of the mind," as Mr Lowe calls it, considerable advances have been made through members of this Society. In 1824, Dr Otto, of Copenhagen, wrote an "Account of an Instrument for the Treatment of Wounds of the Chest;" in 1826 Mr John Turner, Professor of Surgery to the Royal College of Surgeons of Edinburgh, presented a paper, "On the Causes of some of the Sensations communicated by the Action of the Heart through the Stethoscope or to the Touch;" in 1828, "Observations on the Effects of the Sun's Rays on the Human Skin," by Dr John Davy, Physician to the Forces, and brother to Sir Humphrey; another, by the same author, "On the Specific Gravity of different parts of the Human Body;" another, from the same source, entitled, "On the Effect of Removing Atmospheric Pressure from the Fluids and Solids of the Human Body." We possess an interesting autographic letter from Sir James M'Grigor, addressed to our secretary, on the subject of this paper. In 1829, Dr Davy read to us "Observations on the Colouring Effect of Blood and Bile on the different Textures of the

Human Body." In 1830, he read "Observations to endeavour to ascertain if Dead Animal Matter absorbs Air on exposure to the Atmosphere; and if the Putrefaction of Animal Matter is attended with an Elevation of Temperature;" and he farther, on the same evening, read "Observations on the Maceration in Water of different Textures of the Human Body." In 1838, Mr Scott Russell read a communication styled "Remarks on certain Hydrostatical Phenomena which accompany the Submersion of the Human Body in Water, with some illustrative Experiments;" and thereafter Dr (now Sir Robert) Christison narrated the case of a medical brother whose life he had saved by diving into a pool after him, as illustrating Mr Russell's remarks. In 1840, Dr John Reid mentioned a case of "Diaphragmatic Hernia, the opening four inches in length, through which Stomach and Spleen had passed into the Thorax, with Tympanitic sound on percussion." In 1841, Dr Abercrombie read a communication from Professor James Forbes "On the Acoustic Qualities of different Stethoscopes, with some Remarks on the Principles to be attended to in their Construction." In 1845, Dr Spittal read a paper "On the Friction Sound perceived in Peritonitis, its Mechanism, and Diagnostic Value." In 1845, the late Sir James Simpson read one "On the Artificial Displacements of the Bones of the Feet in Chinese Females, with a Specimen." The same year Dr Warden read one "On the Application of Prismatic Reflection to the Investigation of Disease in the Open Cavities of the Body, with Cases and Exhibition of Instruments;" and Dr Spittal "On his Modification of Chest Callipers." In 1849, Sir James Simpson gave us a paper "On the Air-Tractor as a Substitute for the Midwifery Forceps, with Experiments to show its Power," etc. In 1852, Dr Sellar read a communication "On the production of Vesicular Emphysema in the Lung, as explicable by the Principles of Pneumatics." In 1854, the late Dr Skae read one "On the Weight and Specific Gravity of the Brain;" and Dr William Gairdner, in 1854, read another "On the probable Mechanism of the Systolic Murmur at the Apex without Deformity of the Mitral Valve." In 1863, Dr John Struthers gave a communication "On the relative Weight of the Viscera on the two sides of the Body." Subsequently have been read a paper, in 1864, by your President, "Description of (1) a Day-Light Apartment in use for teaching Microscopic Anatomy, constructed to accommodate twelve Students at a time (with illustrative sketch); (2) Mr James Bryson's vertically-revolving Drum Microscope, for adjusting and viewing clearly, by Day or Lamp Light, twelve Objects of different thickness at one Focus (*shown*); and (3) a Fixed Lever-Tractor in use for Practising Injections and Post-Mortems, to draw apart the lateral halves of the Sternum, so as to expose the Heart and the Roots of the Lungs without division of the Costal Cartilages (*shown*);" and papers in 1866 "On Galvano-puncture in Aneurism," and on the "Diffusion Figures of Liquids, each illustrated by the Oxyhydrogen Microscope," by Dr Strethill Wright. Further, there are on record, in 1865, a paper by Dr John Smith, jun., "On certain Conditions of the Head as a cause of Syncope;" by Dr George Balfour, "Notes on the Polarization of Light by Crystals of Oxalate of Lime;" and one, "Notes on the Retentive Power of the Abdomen," by Dr Matthews Duncan; and two, in 1868, by the same author, one "On Examination of the Bladder," and another "On the Uterus as merely fixed by Adhesions." We have, in 1869, a communication on "Artificial Respiration by the Sylvester Method," by Staff-Surgeon Jessop; and one "On the Action of Light on the Pupil," by Dr Argyll Robertson; a paper, in 1871, "On Sunstroke," by Dr Thin, of Shanghai; one, in 1873, entitled a "New Method of Determining the Anomalies of Refraction," by Dr Laidlaw Purves; and two lectures were delivered by Dr Arthur Gamgee, during the same year, within the Royal College of Physicians, "On the Processes of the Animal Body concerned in the Development, Distribution, and Regulation of its Heat."

Farther, at a social, scientific, and philosophical *Réunion,* held lately at Freemasons' Hall,[1] "Dr M'Kendrick exhibited a splendid set of Apparatus for producing Artificial Respiration, worked by Electro-Magnetism, and a section of a Cat's Ear in connection with an Apparatus showing the Movements of the Bones of the Ear in Hearing;" while "Dr Gamgee showed several Galvanometers; also the Spectrum of Blood, and a great number of extremely beautiful transparencies, prepared by Dr Strethill Wright, of Sponges, showing the Cohesion Figures and the Structure of Polypes and Infusoria; together with an Apparatus for counting the number of Electric Shocks transmitted to a Nerve or Muscle, in order to produce Tetanus."

II. *Chemistry.*

It is well known that some original investigations in CHEMISTRY were successfully conducted by certain members of our Society. In 1828, Dr Davy read an "Account of some Experiments on different parts of the Human Body—Effects of Dessication, Tannin, and Corrosive Sublimate." In 1829, he read three papers, named, respectively, "Farther Observations and Experiments on the Blood;" "On the Action of Vinegar on the Textures of the Human Body;" and "On the Temperature of the Body after Death; and Experiments made in search of Air in the Joints and Sheaths of Tendons." Sir Robert Christison read, in 1824, "Observations on the different Processes at present employed for the Detection of Minute Quantities of Arsenic in Mixed Fluids;" in 1825, "Experiments on the Evolution of Carbonic Acid from the Blood during

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[1] In answer to 930 invitations to this Conversazione that were issued, above 500 visitors were present,—including representatives of the various professions and public bodies,—to participate in the "*Illustrations*" that were presented "*of the Progress and Present State of Medical Science.*"

Coagulation;" and in 1828 and 1830, "Observations and Experiments on the Causes of the occasional Milky Appearance of Serum," about 3½ per cent. of Oil, obtained from Human Blood. Dr Douglas Maclagan, in 1840, read a paper "On the Action of Hydrated Sesquioxide of Iron on Arsenic;" and, in 1841, "Observations on the Composition of some Intestinal Concretions." In 1842, Dr George Wilson gave "A Chemical Analysis of the Fluid ejected from the Stomach, that contained immense numbers of Sarcinæ;" and Dr Christison gave the "Analysis of a *Nostrum* for Stuffing decayed Teeth." In 1852, Dr Martin Barry read a communication "On the new Process of Baron Liebeg for determining the quantity of Urea and Common Salt in Urine." In 1858, were read "Notes of two Cases in which Urine yielded Indigo," by Dr Carter. Dr Thomas Fraser, in 1867, read "Some Remarks on the Action of Galvanism on Blood and on Albuminous Fluids;" and the same author, in 1868, read a paper entitled, "On the Connection between Chemical Constitution and Physiological Action, with special reference to the Physiological Action of the Salts of the Ammonium Bases derived from certain Vegetable Alkaloids."

At the Conversazione (in Freemasons' Hall) Dr Fraser exhibited specimens prepared by himself of the vegetable alkaloid "Digitaline;" and Mr Dewar exhibited the peculiar properties of Palladium, a very rare metal, which is found in small quantities along with Platinum, and which possesses the remarkable character of absorbing hydrogen. Among other interesting experiments, Mr Dewar showed the working of a peat filter, which had clarified the worst of the stuffs that came from the works at Galashiels, and was seen in the process of clarifying porter.

III. *Zoology*.

Under this head the number of original papers is considerable, commencing with that of Dr Robert Grant on the Anatomy of the Abdominal Viscera of the Sword-fish (*Xyphias gladius*), and read by him in 1826. In 1834, Dr John Reid read "Observations on the Structure of the Mesenteric Glands in the Balænoptera Rostrata." In 1836 we have a "Notice," by the late Dr Knox, "respecting the Occurrence of Entozoa (*Vibrio Trichina*) in the Human Subject; specimens exhibited." In 1837 Dr Grant gave a description of the Lernæa, a parasite found on the cornea of the shark. This was followed up by the late Mr John Goodsir in 1841, who gave a "Demonstration of the Trichina Spiralis, an Entozoon infesting the Muscles of the Human Body;" and by Dr Cormack, who showed the "Larva of a Beetle passed from the Bowels of a Child. In 1844, Mr Harry Goodsir gave an account of "The Structure, Economy, and Development of a hitherto undescribed Entozoon, which had destroyed the Textures of the Abdominal Viscera" in a case described by Dr John Gairdner. The same lamented naturalist the same year read an "Account of the Structure and Functions of the Hydatids" found in the cavity of the Abdomen in a case related by Mr Lee. In 1858, Dr Charles Wilson read "Notes on the Prior Existence of the Castor Fiber in Scotland, and its ancient and present distribution in Europe, and on the Use of Castoreum." Dr Phillippo, of Spanish Town, communicated a paper "On the Bifurcated Heart of the Manatee," in 1861. In 1869, your President read a case of Bovine heteradelphous monstrosity, with a Rudimentary Pelvis illustrative of Arrested Twin Development; with a dissection and drawing." In 1870, your President read a "Notice," and showed specimens, "of Encysted Filaria within the Muscles of the Cod-fish." In 1872, *Dr M'Kendrick* read a paper upon the "Results of Experiments upon the Corpora Striata, as conducted upon numerous Pigeons;" and the same year *your President* read a "Notice," and showed specimens, "of Filaria Piscium within the muscles of a Salmon current in the market."

At the Conversazione, among numerous exhibits illustrative of the Zoology of the sea-beach, an ancient smooth "Sea Anemone," or Actinia mesembryanthemum, exposed to view by *Dr James M'Bain*, R.N., was specially worthy of notice. "This marine organism was taken from a rock-pool at North Berwick, in August 1828, by the late Sir John Graham Dalzell, in whose possession it remained till his death in 1851, giving birth during that time to 344 young Actinias. The Actinia, or 'Grannie' (as it had been familiarly named), was then handed over to the late Professor John Fleming, of the New College of Edinburgh, in whose possession it remained until his death in 1857. In the spring of that year, after being unproductive for many years, 'Grannie' unexpectedly gave birth, during a single night, to 240 young Actinias. This Actinia was next presented to *Dr M'Bain*, who keeps it in the same small glass jar in which it dwelt with its former owners. Under his care it has produced young ones at six different intervals. In the month of August 1872, 'Grannie' gave birth to upwards of thirty young Actinias, several of which are now living. On the 9th December 1872, whilst the glass jar was being cleaned and the sea-water renewed, 'Grannie' gave birth to nine young Actinias, five of which were alive and growing well on 28th June 1873. Several of the Actinias produced in December 1872 are alive and thriving, in the possession of other naturalists." *Mr Andrew Wilson*, Lecturer on Zoology, exhibited fourteen specimens preserved in spirit, illustrating the form and structure of the Cephalopoda, Elasmobranchii, Anomurous Crustacea, Arenicola piscatorum, Nereis, Ammodytes, Trigla, Lophius piscatorius, Ophiuræ, Sabella, Pecten, Buccinum undatum, Gizzard of Fowl, and Plecotus auritus,—"all as adapted for preservation in the Home Museum."

Mr A. B. Stirling, Assistant-Conservator of the Anatomical Museum of the University, showed a very fine collection of Salmonidæ, illustrating the development of the members of this family, and representing the various stages of existence of the Bull Trout, the Yellow Trout, and the Salmon.

"The collection of Salmonidæ was begun by me in August last, to supply a want in the zoological department of the Anatomical Museum, as well as to illustrate the condition and development of the roe and milt in the male and female parr of Salmo eriox."

"This collection includes:—

"*1st.* A parr of Salmo salar of first year. Weight on 9th August, 3 drachms. From Tweed.

"*2d.* Male and female parr of Salmo salar of second year. Weight on 9th August—male, 7 drachms; female, 5 drachms. From Tweed.

"*3d.* Three parr of Salmo eriox of second year. Weight on 9th August—males, 2½ ounces; females, 2 ounces.

"The males had the milt about three parts grown. Weight of both testes, 1½ drachms. Spermatozoa were forming in the spermatic cells, but none were free. The female had roe about one-fourth grown, measuring one inch in length, and both ovaries weighed about half a drachm. The eggs were quite visible to the naked eye, and under the microscope showed clearly the vitelline membrane, the granular membrane or yolk, and the germinal vesicle; in short, perfectly normal ova in course of development.

"*4th.* Three parr of Salmo eriox. Weight on 23d September—males, 2¾ ounces; females, 2½ ounces. From river Eden, in Fife, six miles above the tideway.

"The males appeared slightly advanced, the milt being 12 grains heavier than those from Tweed on 9th August. The females had roe of precisely the same weight and microscopic appearance as those of 9th August.

"*5th.* A male hybrid. Weight on 9th August, 2¾ ounces. Captured in Tweed near Broughton. This fish has the male organs well developed, but in the filamentous condition, no secretion of spermatozoa having begun in the spermatic cells.

"*6th.* A series of preparations of coecal appendages of Salar, Eriox, Ferox, Fario, and Loch Leven Trout.

"*7th.* Three specimens of Blacktail, one of common Trout (S. Fario), and three parr of Salmo Eriox. These specimens were all captured with fly on 24th September in the river Eden, about two miles from sea, in the estuary, where the water is brackish at all times of the tide.

"The blacktails weighed respectively 3, 6, and 8 ounces each; the parr about 2¾ ounces each.

"Those parr are similar in colour, marking, and condition, to those captured six miles above the tideway on the day previous, and also to those taken in Tweed on 9th August, at least forty miles above the tideway.

"The specimen of common trout, with the exception of the pectoral, ventral, and anal fins, was less yellow than those taken in fresh water of the same river; the red spots were very pale, and the whole body much clearer and more silvery.

"As I intend to continue this subject more fully in another form, I will simply give a few of the conclusions to which I have come from the investigations I have made, and from observation during a period of over forty years as an angler and net-fisher.

"*1st.* I consider the number of vertebræ to be no test of species of salmonidæ, Salar varying from 58 to 60; Eriox, 58 to 60; Ferox, 58 to 60; Fario, 58, 59, 60, to 61.

"*2d.* Pyloric appendages I consider no test, as I find, from examination of a vast number of all species (except Ferox), that the number of appendages overlap; also that those individuals having fewer appendages have them of larger size. The highest I have found in Salar is 70, the lowest 49; in Eriox, 46 to 64; in Ferox (two specimens only), 48 to 58; in Fario, 34 to 61; in Loch Leven trout, 38 to 59.

"*3d.* Vomerine teeth I consider no test. Salar has three or four teeth in the front of the vomer, arranged in the transverse direction, and none in the mesial line. I have specimens of Eriox that have no vomerine teeth at all; generally there are three or four teeth in the front of the vomer, the same as in Salar. The Salmo Levenensis (silvery variety), three or four teeth in the front of the vomer; none in the mesial line. Salmo Fario, a zig-zag row, which appears like a double row along the palate.

"*4th.* Parr.—The mystache, or upper jaw, in all parr proper is constantly shorter than in the yellow trout, reaching only to the middle of the eye. In the common yellow trout, of every size, the mystache extends to the back, or even behind the eye. This test is invariable in the parr and yellow trout. In blacktail it reaches to the back part of the eye, as in bull-trout, grilse, and salmon.

"*5th.* I consider the yellow-fin of the Allan, the orange-fin of the Tweed, and the two-year-old parr of Salmo Eriox, to be identically one and the same species. (See Mr Shaw's experiments.) I also consider the two-year-old parr of Eriox, both males and females, to be functionally adult fish as regards the power of procreation—that they are capable of continuing, and do continue, their species in the small as efficiently as in the large stage. As size is nothing in a physiological point of view as regards the function of procreation, there is nothing surprising in the fact that fish of two years old should be capable of continuing their species, seeing that many of our domestic and wild animals breed at much earlier periods of their lives—cats and dogs at nine months, rabbits and rats at six months, sheep and pigs at twelve months, and the cow at two years. The blacktail, the lammasman, and phinock, I consider the same fish under different names in different localities; also that they are barren during this phase of their lives.[1] The fact of their having bred prior to this stage may be a provision of nature to facilitate that rapid growth which is known to occur between the blacktail and the so-called bull-trout, ranging in twelve or fifteen months from ten ounces to five or six pounds.

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[1] See evidence of Mr Croall, superintendent of Perth Fishery Board, and others, in Napier *versus* Peach, Stirling, 1871. Verdict of Sheriff Sconce for defendant.

"The blacktail may be captured in the estuary of the Eden from the beginning of March to November. I have frequently captured them during those months, at intervals, for the last thirty-five years, and also female parr with fully-developed roe during the autumn months.

"The Salmo Eriox, under the name of bull-trout, has got a bad character, and efforts are being made by some proprietors to exterminate him in their rivers, with the view of increasing the breed of Salmo Salar, the young of which he is said to devour in the roe and parr stages; and no doubt he is guilty of indulging in both of those dainties, and also of those of his own family; but is Salar himself not equally guilty of devouring both the roe and the juvenile members of his own family, as well as those of Eriox? I believe him to be equally destructive; and while admitting him to be the king and sovereign of the race, and worthy of every reasonable effort for his protection and multiplication, still I consider Eriox to be a prince of the blood, and second only in the feast, in the chase, and in commerce. A determined and successful explorer, and capital breeder, may he defy all efforts to exterminate him, and be ever at the right hand of his jolly namesake and glorious companion, John Bull."

Mr Stirling further exhibited a series of specimens illustrating the art of making Microscopic sections of an entire animal, retaining the organs *in situ*.

"The series of specimens illustrating the art of making thin sections of soft tissues for observation with the microscope, was prepared by me from a fœtal rat, $\frac{7}{8}$ths of an inch in length, and $\frac{3}{8}$ths of an inch in thickness.

"Beginning at the left side, 108 sections were made consecutively in the long axis of the body. The first fifty-eight of these were rejected as containing nothing of importance, and the succeeding fifty mounted separately, with all the organs retained in their natural position. The eye has been nine times divided, and appears in eleven of the sections. The tail, along with the spinal column, has been four times divided in its entire length, and appears in six of the sections, giving views of the entire animal from the tip of the nose to the tip of the tail. Only four sections of the fifty mounted are beyond the middle line of the fœtus, making each section about the 200th of an inch in thickness.

"The sections were cut by means of an ordinary razor, with the aid of my section-cutter."[1]

Dr George Balfour gave a polariscope view at 480 diam. of the shell from Ichaboe guano, and under a low power a transverse section of the horny lamellæ, etc., of the hoof of a horse. Dr Strethill Wright sent a collection of his diagrams, showing the Lower Organisms as they appear under the Microscope. The diagrams were cut in coloured glass, and came out very clearly, as they were disposed with brilliant lights behind.

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[1] For description and method of use see Journal of Anatomy for May 1870.

Your President showed, under a vertically-revolving Drum Microscope, at 120 diam., injections of the Sow's uterine mucous membrane and the Cat's spinal cord; the Lobster's pulmonic mucous surface, and the Sarcoptix Scabiei. He showed, also, at 480 diam., a polariscope view of the Sarcinæ Ventriculi; remarkable Crania of a Walrus, from Davis Straits, and a Gorilla, from Gaboon River, together with drawings of natural size, enlarged views, and microscopic ones, illustrative of the anatomy of a new species of Polyodon (*P. gladius*), taken from the Yang-tsze-kiang, 450 miles above Woosung.

Dr Keiller exhibited large Wax Models, illustrating the Embryogeny and Development of the Chick, beginning with the descent of the impregnated ovum in the oviduct, and showing its calcification and ejection from the cloaca. The series of twenty-one Eggs, displays the gradual formation of the Chick, until its complete development and hatching.

IV. *Anatomy.*

A. NORMAL ANATOMY.—The well-known anatomical paper by *Dr Andrew Duncan, jun.*, "On the Arrangement of the Fibres of the Ventricles of the Heart," was read in 1827; and then, "*Sir George Mackenzie's* Paper on Embalming." *Mr Alexander Watson*, in 1828, followed, "On certain Nervous Filaments which pass directly from the Ophthalmic Ganglion to the Optic Nerve." In same year, *Dr John Davy* read two papers, *one*, "Account of a New Method of making Anatomical Preparations;" *another*, "On a Peculiarity in the Structure of the Ductus Communis Choledochus, and of the Pancreatic Duct in Man." In 1835, *Dr Allen Thomson* gave an "Account of Dissection of a Pregnant Uterus between the Fifth and Sixth Month, in reference to the Vascular Connexion of the Decidua and Placenta." The following papers followed:—In 1841, *Dr J. Y. Simpson*, "Are Compound Parts Regenerated in the Human Body?" In 1842, *Dr Allen Thomson*, "On the Healthy Structure of the Intestinal Glands, with exhibition of Preparations;" and *Mr Spence*, "On the Eighth Pair of Nerves, with a Dissection showing the source of the Motor Filaments of the Vagus." In 1844, *Mr Syme*, "Evidence in favour of the Periosteum forming New Bone." *Mr Goodsir*, "On the Mode of Reproduction after the Death of the Shaft of a Long Bone." In 1845, *Mr Alexander Watson*, "Observations on the Formation of Bone by the Periosteum,—Illustrative Preparations exhibited under the Microscope." In 1846, *Mr Goodsir*, "Notice of Observations on the Thyroid, Thymus, and Suprarenal Bodies." In 1848, *Mr E.*

Crisp, "On the Non-Muscularity of Arteries." In 1849, *Dr John Struthers,* "Short Notice of Observations on the Muscles of the Eye." In 1853, *Mr Goodsir,* "On a New Method of Enclosing and Covering Wet Preparations, with Remarks on the Details of the Process." In 1854, *Dr Robert Reid* communicated "Observations on the Dentition of the Aztec Children recently exhibited in Edinburgh." In 1857, *Dr Struthers,* "On the Round Ligament of the Hip-Joint. In 1858, *Andreas Retzius, of Stockholm,* "Some Remarks on the Proper Design of the Semilunar Lines of Douglas." In 1866, *Dr W. Macdonald,* "On the Fœtal Circulation." In 1872, *Drs Sanders* and *Tuke, Mr Turner,* and *your President,* "Report on the Exact Anatomical Seat of the Cerebral Lesion in a Case recently narrated by Dr Tuke."

At the Conversazione, *Mr Spence* exhibited some beautifully dissected and dried specimens, prepared by himself, of the Anatomy of the Hand and Foot, exposing to view their minutely-injected arteries; while *your President* showed, under miscroscopes 160 to 400 diam., the structure of the Sympathetic and Spino-Cerebral Systems, the Organs of Sense in the Fœtal Structure, etc.

B. ABNORMAL ANATOMY.—In 1822, "a paper was read by *Dr Andrew Duncan, jun.,* on a Case of Bifid Brain." In 1823, *Dr Holmes, of Montreal,* communicated to the Society a "Case of Unusual Malformation of the Heart."[1] In 1824, *Dr Wardrop,* Surgeon to the King, read "An Accurate Account of A-ké, a Chinese boy, who exhibits the *Lusus Naturæ* of a Parasite Child attached to the Abdomen, by an Eye-Witness." The same year, *Dr Andrew Barry* read "An Account of a *Lusus Naturæ,* in which two children lived for years, united by the Epigastric Region; with a Drawing." Farther, in 1824, *Dr Hastings* gave a "Case of Monstrous Production, without Upper or Lower Extremities; with a Drawing." In 1825, *Mr John Crooks, of Kilmarnock,* read a "Case in which the Stomach of an Infant terminated in a Cul-de-sac; with a Preparation." Other remarkable papers follow, viz.:—In 1827, by *Dr Moncrieff,* "Case in which, on Dissection of a Child of four years, Transposition of Viscera of Thorax and Abdomen was discovered." In 1834, *your President* read an account of a "Case of Hypospadia, with Cleft Scrotum, *believed a female* till thirty-three years of age, and exhibited two Casts (now within the R. C. S. E. Museum) which he had made of this person." In 1835, *Mr George Glover* gave a "Case of great Enlargement of the Ureters, with Malformation of the Bladder, in a male child, the Dissection and Coloured Cast by *your President.*" In 1836, *Dr Knox* made "Observations on a Case of Congenital Malformation of certain of the Abdominal Viscera, more especially the Kidneys and Large Intestines;" and in 1839 *Mr Glover* read a "Case of Malformation of the Head, commonly called an Acephalous Fœtus, the Dissection and Cast by *your President;*" and *the latter* read a

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[1] With a drawing.

paper "On Extrophy of the Urinary Bladder, exhibiting an aggravated case of the same in a gardener aged forty years." In 1840, *Dr John Scott* gave a "Case of Open Foramen Ovale." In 1841, *Dr Cormack* showed "a Drawing of a Man in whom the Ureters opened at a single orifice over the Pubes." In 1842, *your President* read a paper "On certain Deviations from the Natural Structure of the Encephalon, with the Dissection of an Encephalocele, and a classification of Connate Deficiencies of the Encephalon;" and *Mr Spence* "exhibited a much enlarged Right Kidney, whilst on the *left* side the Kidney and Ureter were *wanting*." In 1843, *Dr Allen Thomson* read, "On the Malformation of the Upper Jaw." In 1844, *Dr Ransford* gave a "Case of Malformation of the Rectum;" and in that year *your President* gave as a title to some anatomical observations, "On some Deviations from Normal Anatomy that bear on Practical Surgery." In 1845, "Notice of some Cases of Congenital Fissure in the Neck," by *Dr Gairdner* and *Professor Allen Thomson*. In 1849, "*Dr Alexander Wood* exhibited a child of fourteen days old, in whom the Thumbs were wanting, and the other Bones of the Hand were longer than usual." *Dr Mackay* "exhibited a Child with Extrophy of the Bladder." In 1855, *Mr A. M. Edwards*, "On some Points in the Anatomy of Talipes Varus." In 1858, M. Groux, the subject of "Congenital Fissure of the Sternum," was introduced, and "Observations on his remarkable Abnormality" made by *Professor Bennett*. In 1860, "A nephew of Rokitanski, Herr Wojaczek, from Vienna, having a Malformation of the Chest, the distance between the lower end of the Sternum and the Spine being only one inch and a half," was introduced to the Society by *Dr Alexander Simpson*. In 1861, "Case of Encephalocele," by *Dr Matthews Duncan*. In 1864, "On some Malformations of the Organs of Generation," by *William Turner*, M.B., F.R.C.S. In 1865, *your President* narrated to the Society the result of his examination of the remarkable "Case of Arrested Twin Development," Jean Battista Dos Santos, who had been exhibited to the members of the Society. In 1867, *your President* read a "Description of a Cyclocephalian Form of the Etmocephaloids, born alive," with two illustrative drawings. In 1868, was read a "Case of Congenital Malformation of the Œsophagus," by *Thomas Annandale, Esq.* In 1870, *Drs Sanders* and *Pettigrew* gave the Society a conjoint paper entitled "Dissection of a Monstrous Fœtus, with Distended Abdomen." In 1871, *Dr Matthews Duncan* read a "Note of a Rare Malformation." In 1872, *your President* read, "Cases of Quadruple Mammæ, the two Lower Rudimentary, in two Adult Brothers, illustrated by Figures." *Mr F. Imlach* gave a "Dissection of a Malformed Fœtus, with Deficient Anterior Abdominal Wall;" and *your President*, the notice of a "Case of Abnormal Termination of the Rectum, with Remarks on the Various Forms of this Deviation." At a subsequent meeting, *Dr Thomas Graham, of Paisley,* gave a notice of a "Case of Hypospadia, *believed a female* till sixteen years of age;" and *your President* appended to Dr Graham's letter such Anatomical remarks on the Morphology of the Inguinal Canal and the External Genitals as his examination of the case had suggested.

At the Conversazione, *Dr John Moir* exhibited a drawing received from St Petersburg in 1857, of twin females united at the vertex, and which lived two months. The Rev. Edward Lane, English Episcopal clergyman at St Petersburg, in his letter to Dr Moir, states that he saw them alive in April 1855. *Dr Keiller* showed, 1*st*, united fœtuses resembling the "Siamese Twins," the bond of union in this case being of a somewhat similar nature, but uniting the "twins" in a more complete manner. They were born in Edinburgh, and appear to have reached the fourth month of uterine life. 2*dly*, a model of "the Siamese Twins;" and 3*dly*, photographs of "the African twins," in the nude state, and therefore showing the peculiarity of their mode of union, which was limited to the lower dorsal and sacral region And *your President* exhibited a *cast* of the Foot of a Chinese lady, sent to him by Dr Kincaid, Surgeon, R.N.

V. *Physiology.*

Our knowledge of the facts of healthy life—*i.e.*, an acquaintance with the doctrines of life and function—serves us in our profession as a principle; one of the advantages of proceeding on which, over proceeding on the temporary devices of our progenitors, who contented themselves with practising from experience only, is that, so far as our physiological doctrines are proved to be true, such a principle is more or less good throughout all the healing art.

Our earliest paper is one by *Dr Chas. J. B. Williams*, now of London, and read in 1824, with the title, "Observations on the prevailing Theories regarding Respiration and Animal Heat, with Experiments." In 1825, *Dr Alison* read his first well-known paper, "Observations on the Physiological Principles of Sympathy, particularly in reference to the Doctrines of Mr Charles Bell." *Dr John Dill* followed with an "Essay on Cutaneous Absorption, with Experiments." In 1827, the well-appreciated "Notice of some Experiments on the Mode in which Death is produced by Asphyxia," was communicated by *Dr J. P. Kay* (now Sir James Kay Shuttleworth). Thereafter, *Mr Alexander Watson* read his paper on "The Loss of the Power of Speech from a Blow upon the Head." In 1834, *Dr John Reid* read an "Account of Experiments to ascertain the Influence of the Eustachian Valve in the Fœtus." In 1835, *Dr Douglas Maclagan* gave an "Account of a recent instance of Total Privation of Food for Twenty-three Days, with a notice of similar Cases." In 1838, *your President* communicated to the Society his "Theory of the Cause of Death after the *rapid* entrance of Air *in large quantity* into the Veins."

In 1840, *he* also read a paper, "On a remarkable Diminution of the Medulla Oblongata and adjacent Portion of the Spinal Cord, unattended by any Symptoms of Paralysis." In 1841, *Dr W. Williamson* narrated a "Case of Abnormal Menstruation;" and the late *Dr James Hunter's* MSS. were found to contain a paper "On the Defective Vision of the Albino," which was read by Mr Walker. In 1848, *Dr Gunning* gave a paper "On the Physiological Action of Chloroform, with Experiments;" and *Dr J. Y. Simpson* "On the Positions (natural and preternatural) of the Fœtus *in utero*, as resulting from the action of the Reflex System." In 1850, *Dr William Robertson* read the "Notice of a Case in which a communication between the Stomach and Surface of the Abdomen had existed for several years." In 1851, *Dr Alexander Wood* read "Contributions towards the Study of certain Phenomena which have recently been denominated Experiments in Electro-Biology." In 1853, *he* gave "Practical Deductions from Physiological Laws in regard to Diseases where the properties of the Moving Fibre are implicated." In 1859, *Dr Alexander Wood* read a "Case of curious Physiological Symptoms in connexion with Tumour in the Brain." In 1862, *Dr Arthur Mitchell* read a communication on "Marriages of Consanguinity, their influence on Offspring." In 1865, *Dr Matthews Duncan* read a paper, "On Some of the Laws of the Production of Twins."

At the Conversazione "*Dr Chiene* exhibited an *Apparatus, closely resembling Stricker's hot stage*, for keeping animal fluids, as blood, pus, etc., alive, whilst under Microscopic Examination. Although rudely constructed, in order to keep down expense, it is quite efficient, and it can be made for six shillings, exclusive of thermometer. The instrument is made to suit the Clinical Thermometer in common use." *Dr M'Kendrick* "exhibited a splendid set of Apparatus, showing artificial respiration worked by electromagnetism; the movements of the bones of the ear; the circulation of the blood in the tail of a living fish; for counting the number of electric shocks transmitted to a nerve or muscle in order to produce tetanus; the ciliary action in the gill of the mussel and in the cell of a frog's tongue." Lastly, *Dr Bell Pettigrew* "exhibited fifty preparations illustrating the structure and physiology of the muscles and nerves of the heart. These were representative specimens of a large collection, amounting to nearly two hundred, prepared by this gentleman, and deposited in the Anatomical Museum of the University of Edinburgh. They embraced dissections of the heart of the fish, reptile, bird, and mammal. He also exhibited some ingenious models, which enabled him to show the heart as a transparent object. By this aid he demonstrated that the heart (ventricles) represented a mathematical problem of great complexity and beauty; its ultimate fibres being arranged in the form of a double conical screw—the one screw winding from right to left on the outside, and from left to right on the inside, of the heart; the other screw winding from left to right on the outside, and from right to left on the inside, of the heart. The screws intertwine at the apex and base of the organ. This arrangement, Dr Pettigrew explained, enables the heart to wring out its blood when it contracts, so that the phrase, wringing the heart's blood, is less metaphorical than literal. The effects produced by the spiral arrangement of the fibres is well marked, not only as regards the wringing out of the blood, but also with reference to the movements of the heart as a whole. Thus, when the ventricles expand, they screw their apices deeper into the thorax, precisely in the same way that a screw-nail is introduced into wood. When the ventricles contract, the apices are unscrewed and elevated, as happens when a screw-nail is withdrawn from wood. In addition to the spiral arrangement of the fibres described, the fibres are arranged at right angles and obliquely, in seven layers, to produce a minutely-reticulated structure, which is very powerful, from the fact that it is supported in every direction by cross fibres, on the principle of the girder bridges seen on railways. Thus, the fibres of the first layer proceed from left to right, and are nearly vertical; those of the second layer being slightly more oblique, and those of the third still more so. The fibres of the fourth layer are disposed at right angles to those of the first layer. When the fibres of the fourth layer are passed, the fibres of the remaining layers—viz., five, six, and seven—gradually return to the vertical in an inverse order, from which it follows that the first and seventh layers cross, with a slight deviation from the vertical, as in the letter X; those of the succeeding layers, until the fourth or central one is reached, crossing more obliquely, as in the letter X placed horizontally—so, ⋈. This arrangement, as Dr Pettigrew pointed out, invests the ventricles with a centrifugal and centripetal action; the fibres elongating simultaneously when the ventricles expand, and shortening when they contract. But for this consentaneous movement of all the fibres when the organ opens and closes, the heart could neither receive nor eject its blood. The centrifugal and centripetal action of muscle here referred to is significant when taken in connexion with Dr Pettigrew's original views, recently published in this Journal (January and February of 1873), as to the rhythmic movements of the chest and abdomen in respiration; the hollow involuntary muscles (heart, stomach, bladder, uterus, etc.), as he expressed it, being the precursors, and affording the type or pattern on which the voluntary muscles of the chest are formed and act,—a remark which, he observed, also applies to the voluntary muscles of the extremities. Dr Pettigrew pointed out the remarkable fact—as interesting as it is novel—that the muscles of the ventricles are arranged at various degrees of obliquity around spiral cavities, and that the muscles of the extremities are arranged in a precisely similar manner around spiral bones. The nerves of the heart, as Dr Pettigrew showed, are exceedingly numerous, and studded with ganglia. The nerves, curiously enough, also run in spirals; the spiral lines formed by the nerves intersecting those formed by the muscular fibres at nearly right angles. The nerves are consequently disposed in such a manner that they can stimulate the muscular fibres at innumerable points to insure uniformity of action, both when the heart dilates and when it contracts. The nerves flow upon the heart in four principal streams,—an anterior, a posterior, and two lateral. The anterior and posterior streams are the largest. The ganglia are found in all parts of the heart. They are especially numerous on the posterior coronary sinus, and along the groove which separates the auricles from the ventricles; and this may explain why the heart of the frog ceases to pulsate if this region be injured. The ganglia are also found in the track of the anterior and posterior coronary arteries, and that of the larger vessels generally. They are, however, not confined to these situations, and exist in quantity on the surface and in the substance of the heart, as already stated. The number of nerves and ganglia seen in Dr Pettigrew's dissections greatly exceeds that usually represented in books. This is especially true of the ganglionic masses found on the coronary sinus and grooves. The ganglia, according to Dr Pettigrew, are crowded with unipolar and bipolar nerve-cells. The nerve dissections embraced, among others, specimens of the cardiac nerves of man, the horse, ox, sheep, the cat, rabbit, etc. The nerves are all *in situ;* and, in this respect, Dr Pettigrew's dissections differ widely from the cardiac-nerve dissections of Dr Robert Lee, deposited, we understand, in the Museum of St George's Hospital, London."

(*To be continued.*)

ARTICLE II.—LISTER'S METHOD OF THE TREATMENT OF WOUNDS.

Einige Worte zum Verständniss der Lister'schen Methode der Wundbehandlung. Von Dr L. LESSER, Privat Docent in Surgery, formerly in Greifswald.[1] Abridged and translated from the German by WILLIAM STIRLING, D.Sc., M.B., etc.[2]

AFTER mentioning the circumstance that a visit of several weeks to Edinburgh, during last summer session, had convinced him of the great value of the antiseptic system of treatment, and inspired him with a desire to convey the same impression to others, the author proceeds:—

It is to be regretted that Lister's antiseptic treatment of wounds has been confounded with the treatment of wounds by antiseptic substances. The two things have nothing in common, beyond the fact that certain chemical materials render inactive the germs which cause putrefaction in wounds. But this misconception is the reason why, both in England and in Germany, general views rather than fixed principles for the treatment of wounds are connected with the name of Joseph Lister. Every one prepares, according to his taste, a mode of treatment with antiseptics, and imagines that it is superfluous to follow Lister's method strictly; that the various points of his procedure are individual peculiarities rather than postulates for the success of the treatment. How can any one thus succeed in obtaining impartial proof of the results of the antiseptic method?

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[1] Deutsche Zeitschrift f. Chirurgie, B. III., p. 402.

[2] At Dr Lesser's request, Mr Lister has looked over this translation, to ensure accuracy in the statement of facts.

Before all things, we must dispel the error that the antiseptic treatment stands in complete opposition to the open treatment of wounds. Success in both methods is rooted in the same cause, and Lister's method, paradoxical though it may seem, is only a perfection of the open treatment of wounds, and not at all a method of occlusion. Lister allows the air free access to wounds, but only after it has been deprived of its organic impurities. Enveloped in an antiseptic dressing, a wound free from putrefactive germs is protected from the danger of infection, its healing rendered independent of the influence of external noxious agents, and of the locality where the patient may be. Lister divides the space of his wards, as it were, into a large number of small sick-rooms in miniature, whose salubrity no longer depends upon external conditions, but exclusively on the carefulness of the surgeon.

We are at present enabled, by antiseptic dressing, to bring wounds, be they accidental or operative, approximately to the condition of subcutaneous injuries; and, by the removal of all stimulation, to avoid with certainty as well septic as antiseptic inflammation and suppuration, and so arrive at a sort of *prima intentio* in a wider sense. Putrid stimulants are putrefying discharges of wounds; antiseptic stimulants are those chemical substances which we employ to destroy the action of the excitors of decomposition. In the action of carbolic acid, as such, there is nothing specific. It is the mode of employment which produces the admirable results.

To recognise the full value of the antiseptic method, we must consider its relation to the two chief groups of injuries, viz. :—

1. Wounds free from infection, produced either by the knife of the surgeon or by accidental violence—aseptic wounds.

2. Infected wounds, with putrefaction of the discharges, or in which putrefaction of the tissues has already occurred, with general reaction in the organism.

In wounds already infected, the antiseptic treatment, as such, accomplishes but little. Here we treat according to ordinary antiseptic principles. By free evacuation and disinfection of the discharges, we seek to correct their decomposition, and consequently the reaction of putrid materials on the injured tissues. Lister's principles come here only so far into the question, that when putrid surfaces of wounds have been purified of decomposed discharges and putrid particles of tissue, he treats them from that time forward as aseptic, instead of maltreating them on-and-on with first one means of stimulation and then another. In sinuous wounds of cavities, with decomposition of the secretions in their recesses, Lister prefers, by drainage, to promote the passage of the secretion to the surface of the skin, and here first to undertake disinfection. The washing out of the cavities of wounds produces only an unnecessary stimulation of the walls of the cavity. In one case only does Lister employ such injections, viz., in recent complicated injuries, where the fear exists that putrefactive germs have already penetrated into the wound, yet a hope is entertained that the case may possibly be treated as an aseptic one. Here a twenty per cent. alcoholic solution of carbolic acid is carefully injected into the interstices of the tissues. The experiment often fails, more especially where large quantities of air have penetrated into the tissues. Thus it may happen that, though the wound after the injection may remain free from putrefaction, a putrid abscess may be formed at a considerable distance from it, owing to the wandering of the emphysema. Lastly, when we have to do with putrid wounds which have already led to a general reaction, the antiseptic method cannot effect more than the open treatment. Only in the interest of the surroundings of the patient is the freely outflowing discharge disinfected. We see that, in the question of infected wounds treated antiseptically, we can only speak of probabilities, and in several cases of the possibility of removing putrefaction, and so rendering the wound aseptic. The more pronounced the picture, the more menacing the symptoms, the less powerful our antiseptics prove themselves. Here lie the limits of the antiseptic method; against septicæmia and pyæmia it can do quite as little as all the other modes of treatment. Simple cleanliness of the wounds, careful removal of all decomposing matter, may greatly improve the salubrity of an hospital. But the complete extinction of accidental surgical affections of wounds—above all, of pyæmia and septicæmia—is a postulate which is scarcely to be reached where the antiseptic method, as Lister has shown, is not strictly carried out. In opposition to this fact, one often hears the observation that, under such favourable conditions as exist in Lister's wards, scarcely a patient should die. I believe the best reply to such a remark is, Do all the patients in hospitals where pyæmia and septicæmia are the order of the day die of these accidental affections?

But the true triumph of the antiseptic method consists in the *treatment of aseptic wounds.* Here it affords the greatest perfection that surgical art can desire, viz., the repair of injuries by primary formation of stable tissues, without previous granulation or formation of pus, healing taking place by the first intention as occurs in simple fractures. According to Lister, the formation of pus always follows the formation of granulations. Suppuration is only the consequence of a stimulation higher than is necessary for the production of granulations. In each case the stimulant must act for a considerable time on the tissues. If the stimulant, however, acts only once, and that for a short time, as a mechanical stimulant in the infliction of a wound, the tissues respond by simple regeneration, by filling the defect with living organized matter.

The *external stimuli* which disturb the normal healing process by the formation of inflammatory transient tissues are in general the following:—

(*a*.) *Mechanical stimuli*, as tension from accumulation of discharges, pressure from bandages, or traction of sutures.

(*b*.) *Chemical stimuli*, as acids, alkalies, etc. Of course, this group includes antiseptic substances which, by their action on the tissues, excite "antiseptic suppuration;" and

(*c*.) *Septic stimuli*, *i.e.*, decomposed fluids of wounds (putrid tissue juice, blood, or pus), which conduct themselves in their local action just as the chemical substances, but may further act deleteriously by absorption and infection.

What means do we possess to remove all these stimuli from wounds? Amongst these is the antiseptic gauze, which is employed in three ways. First, in the form of plates, formed of eight layers of the gauze, which must extend beyond the wound in every direction to a degree proportioned to the quantity of discharge. In large wounds on the extremities (amputation, resection) it is better to envelop the whole circumference of the limb. In operations on the mamma, two plates are used, of which one encloses the lateral wall of the thorax and the corresponding region of the breast, and the other plate covers the entire shoulder and upper arm, whilst the armpit is padded with loose gauze; and this is the second method of employing it, viz., for the stuffing of cavities and for protecting prominent points of bone from external pressure. Lastly, the gauze serves, in the form of bandages, for fixing the plates, and thus completes the antiseptic purpose of the entire dressing. It is of the greatest importance that air should nowhere have access to the wound, without having first passed through the dressing; therefore the turns of the bandages are not attached loosely, but fixed, especially at the corners of the plates, with safety-pins. An essential adjuvant to such an antiseptic dressing is the *mackintosh* cloth, which is placed between the outermost and the next layer of the plate of gauze, with the intention of forcing the discharge from the wound to diffuse itself in the deeper layers of the gauze, instead of, as would occur without the mackintosh, penetrating rapidly the whole thickness of the dressing towards the atmosphere, and here, where no sure protection against the putrefactive germs is present, putrefying rapidly. This waterproof cloth should nowhere pass beyond or cover the margins of the gauze, so that the discharge may at no place pass between the mackintosh and the skin and putrefy there. Attention to such apparently small details is of essential importance for the success of the treatment.

To prevent the irritant action of the carbolic acid upon the surface of the wound, the oiled silk "protective" (the mode of preparing which is described in an appendix) is employed. This is placed between the carbolic dressings and the wound, and does not allow the acid access to the wound, but the discharge passes from under this protective layer into the porous gauze. The protective can only be applied to surfaces free from putrefaction; of course, it is useless on putrid surfaces. Through the combination of the carbolic gauze and mackintosh with the protective, the antiseptic dressing effects what Lister requires of it, "An antiseptic to exclude putrefaction, with a protective to exclude the antiseptic, will, by their joint action, keep the wound free from abnormal stimulus."[1]

Great care is required in performing operations on uninjured parts to prevent the entrance of putrefactive organisms to the wound. For this purpose, the chief means are the following:—Cleanliness of the hands of the surgeon and his assistants, cleanliness of the instruments, cleanliness of the skin in the region to be operated on, purity of the air about the wound. The instruments are dipped in a 5 per cent. solution of carbolic acid, and the hands of the surgeon, and the skin of the region operated on, are cleansed with the same. But for washing the sponges during the progress of the operation, a 2½ per cent. solution is employed. The bone forceps and the saw are rubbed over with a 10 per cent. solution of carbolic acid in oil, in order to remove all impurities.

For rendering the air about the wound pure, the spray must be employed. It is specially interesting to observe how useful and essential the spray is in opening large abscesses. Lister assumes, that here the formation of pus is caused by inflammatory disturbance kept up by the mechanical irritation of the tension of the pent-up pus; and that, therefore, pus formed in an abscess without entrance of the air, is free from putrefaction. This view is supported on the one hand by the fact, that on opening a large abscess, its contents are expelled with a certain force corresponding to the tension; on the other, that after complete evacuation of the pus (as from a psoas abscess, or a suppurating bursa præpatellaris) under antiseptic precautions, together with provision for the constant free escape of discharge, already in the first twenty-four hours no more pus is produced, but the secretion, with cessation of the tension in the cavity of the wound, becomes serous, and rapidly decreases, giving place to filling of the cavity with stable tissues. The spray also permits the extraction of bodies from the knee-joint by free incision, without subsequent disturbance of the same;[1] and, further, it is of great value when it is necessary to open a joint in consequence of ununited fracture in the vicinity of the articulation, and for operations in the continuity of bones, etc. The spray, indeed, is used in all operations performed under truly antiseptic conditions, and it must be carefully maintained during the stitching of the wound. The author here relates some very striking cases which he saw in Edinburgh. He then cites the circumstances in which it is advisable to employ the spray, and points out, that as yet it cannot be employed with advantage for ovariotomy, on account of the large amount of air to be neutralized.

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[1] Remarks on a Case of Compound Dislocation of the Ankle, with other Injuries.—*The Lancet*, 19th March, 26th March, and 9th April 1870.

We must, however, ask, Is the dust in the air actually the excitor of putrefaction? and if the spray places us in a position to eliminate these noxious agents from the air? The author points out that the experiments of Pasteur are so affirmative for the first question, that it is unnecessary to say more on the matter. Practically, the best proof of the indifference of a pure mixture of the gases of the atmosphere is furnished us, on the one hand, by the pumping out of secretions from cavities under exclusion of air with the aspirator of Dieulafoy; and, on the other, by the evacuation of fluids, under antiseptic precautions, where, however, the atmospheric air, as such, comes freely into contact with the walls of the cavity. Professor Lister has repeated the experiments of Pasteur and sought to give them the simplest form. Lister refers often to the well-known fact, that in simple fracture of a rib, the lung is often wounded by the free fractured end of the bone, so that pneumohæmothorax occurs. The blood poured out into the cavity of the pleura, though it mixes freely with the air, does not putrefy, because here the bronchi and bronchioli, provided that their mucous membrane is healthy, filter out the particles of dust.

Blood poured out in an aseptic wound, treated antiseptically, remains exactly in the same condition, and goes on not to putrefaction but to organization.

Lister employs for the spray a 2½ per cent. solution, but the mixture of air with the carbolic acid during the spraying, must reduce it at last to an antiseptic fluid of considerably diminished strength.

The author then discusses the effects of concentrated solutions of carbolic acid on the tissues, and remarks that the effect of a single application of this substance is not nearly so deleterious as is generally assumed; at least, much less injurious than the fomentation of wounds with a diluted solution of carbolic acid. The impregnation of a wound for twenty-four hours with such a solution does not produce any lasting injurious effect, but by longer application there occurs what Lister has so well termed "antiseptic suppuration." The antiseptic acts just like other chemical substances; and this irritation causes regeneration of the tissues, not in the normal way, by hyperplasia, but by the roundabout way, the formation of granulations; or, lastly, by persistence of the irritation, the regenerative power exhausts itself in the formation of a fluid, quickly-dying tissue, viz., pus. Carbolic acid in a dry condition acts on the unbroken epidermis; and it is sometimes observed that, where the carbolic gauze has been placed on the uninjured skin, after a time erythema, eczema, and excoriations appear. The author further points out that carbolic acid, when used in the form, too often employed, of fomenting the part with watery solution, may exert an injurious influence on the organism, and discusses the symptoms of chronic carbolic-acid poisoning.

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[1] Lister, Address on Surgery, delivered in Plymouth, etc.

In the application of carbolic acid to wounds in the sense of the antiseptic method, and above all for aseptic wounds, we must use a watery solution, for the addition of spirit and glycerine only increases the irritating action of the acid, in that they retain the acid longer, and so hinder the rapid absorption of the carbolic acid from the tissues.

After the application of a strong solution of carbolic acid to a fresh wound, very copious serous exudations occur during the first twenty-four hours, so that a large quantity of dressing is required to prevent the putrefaction of the discharge; whilst it is further necessary to change such dressings at the latest twenty-four hours thereafter; and, thirdly, as free an exit as possible must be provided for discharges from the wound by means of drainage. If these precautions are not attended to, in spite of all care, suppuration may ensue.

Regarding the first point—the large quantity of the dressing material—this is of less importance in hospital practice, because the antiseptic gauze, after being once used, can be boiled and again impregnated with carbolized resin, etc. The second point, the daily changing of the dressings during the first days after the injury, is one of the chief means of promoting Lister's treatment, because, by not following this rule, we can be easily deceived as to the success of the plan; and, secondly, because there are always surgeons who object that Lister's method prevents the surgeon from having sufficient observation as to the condition of the wound. Lister lays special stress upon changing the dressing twenty-four hours after the operation; and, of course, such change must be made under the same antiseptic precautions under which the operation was performed. In the later stages of the case, daily inspection is superfluous, and, as the discharge diminishes, intervals of from two days to a week may be allowed between the periods of dressing.

With regard to the third point, *drainage*, Lister employs caoutchouc tubes of various thicknesses, with correspondingly wide holes. These are kept for some time in a 5 per cent. solution of carbolic acid. When placed in the wound, they ought not to reach beyond the surface of the skin; and in order to prevent their falling into the cavity of the wound, they are made fast to the neighbouring skin by means of two knotted threads impregnated with carbolized wax attached to their outer end. As the discharge diminishes, the tube is shortened. In some cases it is better to have several short drainage-tubes at different parts of the line of incision. Caoutchouc tubes have the great advantage that they imbibe carbolic acid so readily, and become antiseptic in their own substance.

In the daily changing of the dressings, special attention must be directed to the position and permeability of the drainage-tubes, in order to prevent any retention of the discharges of the wound by bending of the tube, or occlusion of it by a coagulum, etc. At the beginning, therefore, the tube should be often changed, in order to prevent the formation of pus. If this, however, should occur, it would be false to depart from the antiseptic treatment. When complete evacuation of the pus has occurred, the tension being removed from the walls of the cavity, and the cause of the formation of pus being thus withdrawn, we may, with the precautions above indicated, expect a pure serous fluid within twenty-four hours. The tubes can be introduced into the wound without pain; and the same tubes may be employed over and over again, even for different patients, as soon as they have been washed in a 5 per cent. solution of carbolic acid.

We must specially notice an exceedingly valuable equivalent of drainage for some cases, as where, in small hollow wounds as after extirpation of a small tumour, complete union by first intention is to be desired. The best method of procedure is to bring together the edges of the wound with sutures, widely separated from each other, and place over this a piece of protective, and then introduce under the gauze plate a piece of sponge which has been washed in a 5 per cent. solution of carbolic acid and squeezed dry. The sponge exercises, on the one hand, a regular and equal compression of the cavity of the wound, and, on the other, promptly imbibes any discharge that may be produced in the wound. It is specially noteworthy that the sponge, which has brought so much misfortune and ill to surgery, should, by treatment with a 5 per cent. solution of carbolic acid, become an excellent aseptic and antiseptic dressing. The use of the sponge may also be often advantageously combined with that of the drainage-tube.

One of the most important chapters in relation to the antiseptic method, is the material to be employed for ligatures and sutures.[1] Lister employs antiseptically-prepared catgut, and silk impregnated with carbolic wax, and also silver wire and horse-hair, for sutures; while for ligatures he employs catgut exclusively. The latter, when it has been suspended for at least two months in a mixture of five parts of olive oil and one part of crystallized carbolic acid, liquefied by the addition of 10 per cent. of water, is not merely antiseptic, but is so altered in physical constitution that it is no longer rendered soft and slippery by the imbibition of fluids; and Lister has demonstrated experimentally that, as well in the human subject as in large animals, catgut within the tissues is partly organized and partly absorbed like blood, sloughs, bone, etc. In deligation of vessels in their continuity, the employment of catgut as a ligature renders it possible to obtain an absolute *prima intentio* (because suppuration does not occur around the ligature under antiseptic treatment), and also renders the place of ligature of the vessel quite independent of the giving off of small branches from the main trunk. We have, in fact, in catgut the ideal of a non-stimulating, secure ligature before us, which causes the dispute between the thread ligature and acupressure, etc., to sink into insignificance.

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[1] Observations on Ligature of Arteries on the Antiseptic System, Lancet, 3d April 1869.

The ends of the catgut are, of course, cut short in the wound. Lister has shown that when such a knot is made with well-prepared catgut round a rod of caoutchouc, and placed for days in putrid serum kept at the temperature of the body, not the smallest relaxation of the knot occurs, so that, even in wounds where putrefaction is present, catgut still remains the best ligaturing material. The security in the holding of the ligature and its non-stimulating properties permit the ligature of all bleeding vessels, without the fear of heaping up too much foreign material in the wound. The exact and minute stanching of hæmorrhage is an important factor for the success of the antiseptic method. And I may remark incidentally, that, besides attending to the most careful stanching of hæmorrhage, Lister provides for the greatest possible economy of blood, in that, after emptying the limb of blood by elevating it to the utmost, a tourniquet is placed round the limb during the whole time of the operation. This method is specially excellent in operations on the extremities; in fact, loss of blood can be almost entirely avoided. Professor Esmarch has also made his German colleagues acquainted with a similar method of treatment. Whilst Lister considers catgut as the best material for ligatures, its easy and rapid absorption prevents its being employed for sutures, at least where there is traction on the edges of the wound. Only as intermediary sutures, or where there is no tension, is catgut serviceable. For plastic operations on the face Lister employs as sutures in preference silver wire and horse-hair, but under ordinary circumstances he uses silk impregnated with carbolic wax. Lister is a great advocate of stitches of relaxation (*Entspannungsnäthe*): they act in opposition to the separation of the edges of the wound, and help to remove the mechanical hindrance to *prima intentio*, and the production of suppuration in the tracks of the sutures.

I have endeavoured, in reviewing the antiseptic method, to depict the means employed with reference to their object and value. But as the antiseptic treatment is no rule of thumb, like so many modes of dressing, but demands for every individual case a scientific knowledge of the circumstances, and, above all, a large amount of practical acquaintance with the normal healing of wounds, it follows that any one to whom the method appears rational can only learn it at the bedside through reflection and experience. He would be grievously deceived who, after the mere study of a written account of Lister's procedure, believed himself at once qualified for the bold operative measures which the Scotch surgeon, in full reliance on the safety of his method, ventures to undertake.

Boracic acid and chloride of zinc are also antiseptics, and are employed as adjuvants in the antiseptic treatment.

Lister employs boracic acid in two forms,—boracic lint, a dressing material, almost non-stimulating, for wounds, where the crystals of the antiseptic in the lint are only dissolved gradually by the discharges of the wound; and a lotion of boracic acid (3 to 5 per cent.), partly for washing and partly as a spray. The antiseptic power is less than that of carbolic acid, and stimulates the tissues less, but, being non-volatile, it is not so evanescent. Lister therefore prefers this substance for superficial wounds, such as those of plastic operations, in the treatment of ulcers, and, lastly, for the purposes of the healing of loose portions of skin on granulating surfaces.

In the case last mentioned, the method is the following:—After the sore is brought into an aseptic condition, it is carefully washed with boracic acid solution, then the pieces of skin to be transplanted are placed upon the surface of the granulations, under protection of the boracic spray. Without fixing specially the small portions of skin, a piece of protective is laid over the wound after it has been dipped in boracic acid solution; upon this a layer or two of boracic lint similarly treated, the whole fixed with a gauze bandage. As the discharge is scanty, the dressing may be left unchanged two, three, or four days.

Chloride of zinc plays a very important part, and enables us to treat as aseptic a whole series of wounds which otherwise must remain in the category of putrefactive. It was specially brought into notice by Campbell De Morgan of Middlesex Hospital. It thoroughly destroys superficial foci of putrefaction, and removes the stench of putrid masses, and forms a sort of eschar by impregnation of the superficial layers of the tissues, which presents an absolute obstacle to putrefaction, and to its extension to the deeper layers. Lister employs a solution 40 grains to the ounce of water, and with the following indications:—

1. For wounds which have been for a longer or shorter time exposed to the influence of atmospheric air, and in which a superficial putrefaction of the tissues has occurred, in order to be able, after destruction of the products of putrefaction and the infected particles of the tissues, to regard the wounds as aseptic. To this class belong superficial surfaces of ulcers, with sloughing of the granulatious and stinking discharges.

2. Also for recent wounds, before putting on the first dressing, when the wound contains some focus of putrefaction, such as sinuses communicating with a joint subjected to excision. In such a case, the sinuses are also injected with the solution, in the hope of correcting the putrefaction of their contents, though the attainment of this is always uncertain.

3. We employ the chloride of zinc for recent wounds in the neighbourhood of the different cavities of the body, and where the continual bathing of the wound with the putrid secretion of the cavity would render infection of the former possible. Chloride of zinc here forms an eschar which, more than any other eschar, resists all putrefactive influences, so that it constitutes an antiseptic covering over the injured tissues, till they are protected by granulation. And it is a capital fact, that such an eschar, even after three days, as in extirpation of a cancroid in the tongue, with absolutely negative treatment, gives rise to no stinking odour. It is right to add, that the eschar caused by this solution of chloride of zinc, though we cannot doubt its existence, must be of microscopic tenuity, being invisible to the naked eye.

In conclusion, the author makes some general remarks, as follow :

To those who have had an opportunity of observing the healing process of wounds under Lister's treatment, it must be clear that the idea of *prima intentio* and *secunda intentio*, as they at present stand, must be modified for the future. When we see in the healing of large open wounds, from the beginning to the end, only tissue-juice trickling out ; when the tender, bluish-white edge of epidermis does not stretch itself over granulating surfaces, but over smooth tissue penetrated by fine bloodvessels ; when we see, in such a wound a blood-clot, which neither shrivels up nor decomposes, but from day to day its organization and vascularization can be followed, partly from the bottom, partly from the walls of the surface of the wound ; and when, lastly, the cicatrix is more resisting and skin-like, without any special tendency to retraction showing itself,—we must confess that, in spite of the many histological papers which have been written of late years on the healing of wounds, entirely novel points of view for further research are presented by the antiseptic treatment.

And what a perspective does Lister's antiseptic treatment open for the operative technique ! The consequences of the employment ot antiseptic catgut for the ligature of vessels we have already spoken of, also the possibility, under corresponding precautions, of opening sound joints without any danger. The brilliant results of opening large abscesses antiseptically makes it our duty to prevent the spontaneous opening of abscesses, and the formation of sinuses in joints, leading to sequestra of bone. It permits us, in acute suppuration of joints, without fear of putrefaction, to substitute evacuation of the pus for resection of the articulation. When there is disease of the articular end of a bone, Lister has obtained good results by only partial resections, but under the condition that the joint remains unopened until the resection, *i.e.*, without putrefaction, so as to permit of the after-treatment of the wound being carried out aseptically.

For private practice, as well as for that in hospital, the difficulties of the antiseptic system are scarcely to be called important. The immediate large consumption of antiseptically-prepared dressing material is compensated for by the less frequent changing of the dressings. The expenditure of time for each patient also appears, on adding up the number of hours so employed, distinctly smaller than by any other method; only the distribution of time is unequal, reaching its maximum during the first few days. In an hospital, however, this is of no moment, because we have at the same time before us patients in different stages of recovery from wounds. And thus we had presented to us what to the uninitiated appeared a very surprising fact, that, with an average of nearly sixty beds, the surgeon only required to renew the dressings in one-fourth or one-fifth of the cases every twenty-four hours.

I need scarcely add that the use of internal remedies, such as quinine, camphor, musk, etc., those cloaks of scientific helplessness with regard to blood-poisoning, is reduced to a minimum, because with the antiseptic treatment we do not allow the enemies of surgical practice to gain access to our patients at all, or nip them in the bud.

The means of carrying out the antiseptic treatment may undergo modification in the future, as they have done in the past; but the antiseptic method itself is not one of the possible modes of managing wounds—it is the only possible one, the only one worthy of modern surgery.

ARTICLE III.—*Case of Extensive Chronic Eczema of the Face and Extremities, of Seven Years' Standing, in a Child, and complicated with Spasmodic Asthma, cured by Pitch, Soft-Soap, Zinc, and the Iodide of Potassium.* By K. N. MACDONALD, M.D. Erlang., L.R.C.P. Lond., and L.R.C.S. Edin.

THIS case presenting as it does several features of clinical interest, as well as affording a practical illustration of the efficacy of simple remedies when methodically applied, not without some interest to the general practitioner, I make no apology in offering it for publication.

So far as I could gather from the parents, the following is a concise history of the case:—W. J. Falconer, now aged eight, of healthy parents following an agricultural pursuit in the Highlands of Perthshire, and third son of a family of five, was born on the 14th of September 1865, and, though small, was apparently a healthy-looking child. On the third day after his birth, however, he sickened, and would make no attempt to take the breast; and on the fourth day, a small rash made its appearance upon his face, which soon faded away, but was succeeded, six weeks after, by another "small thick rash," which first appeared on his forehead and face, and afterwards extended to the head, back of the neck, chest, arms, legs, and thighs, with a spot about the size of a sixpence on each shoulder-blade.

The eruption on the face, after discharging a "clear, sticky fluid," soon became dry, covering the face with a scaly scab like a mask. When three months old, the scabs, dropping off, left an irritable and raw surface, still discharging the same clear liquid above mentioned, which was subsequently treated with bread-and-milk poultices, in consequence of his eyes having been closed for ten days through the disease extending to the eyelids, at which period he was not expected to live. Olive oil and cod-liver oil had also, at the same time, been applied to the other affected parts with more or less relief, and continued during the whole of the winter of 1866; but the irritation was so intense that he required constant watching, day and night, and seldom got more than half an hour's sleep at a time. His hair grew in patches, and had a dry, stunted, and withered look.

When he was about nineteen months old he was kept out of doors for a couple of hours on a cold damp day in April 1867, and in the evening of the same day he was seized with difficulty of breathing, pain in the chest, and a distressing cough, to such an extent that his breathing "sounded almost like a whistle."

This was treated by a warm bath, sinapisms to the chest and throat, with a poultice between the shoulders, which gave considerable relief.

When he was three years of age he took the whooping-cough, from which he recovered fairly; but ever since he has had occasional attacks of shortness of breath, with difficulty of breathing. About this period he got flour of sulphur daily for some months, during which time his head completely cured, without, however, much benefiting the rest of his body. These parts were smeared with a mixture of tar and butter, which apparently caused the disease to disappear for a time; but it was not for long, as the eruption made its appearance again, and was soon as bad as ever. During the above periods he had had sarsaparilla, quinine, and various other internal remedies, without producing any permanent good effect. He has never had measles or scarlet fever, nor has he yet been vaccinated.

Several medical gentlemen, who had seen him from time to time, recommended him to be "let alone," that dentition had had something to do with it, and that it would probably "exhaust itself" after a time—except one, who talked of curing him with arsenic when he grew a little stronger. As regards diet, he had had a trial of all articles that came within their reach, such as corn-flour, sago, arrow-root, bread of different kinds, milk, eggs, and as much cream as he could wish for.

In this way time passed on until he was six years and eight months old, when he first came under my treatment, in May 1872.

The above is far below the pitch of the graphic account which I received from the mother, of the terrible suffering he had endured for years, which made her look back with wonder that she and her son "had lived to see the end of it." Neither of the parents, nor any other member of the family, had ever had any eruptive diseases of the skin, and the only difference between the subject of this paper and his other brothers was, that at the time of his birth his skin in general appeared fairer than that of the others, who were more or less "tawny."

As the case was considered incurable by the parents and friends, I had some difficulty in persuading them that there was nothing at all extraordinary in the case, and at the same time I felt that if my first efforts had failed, the confidence long since lost in all medical treatment would only, in the eyes of those immediately concerned, have been too eagerly established. I therefore counselled patience, strict attention to cleanliness, and a faithful observance of all my directions being carried out to the letter, backed by a suggestion that if he did not improve after a time, it would be quite easy to gain him admission into one of our city hospitals.

Having thus far acted upon the minds of the parents, I found that my patient was indeed a helpless sufferer. His face was completely covered with a thick incrustation, his eyes nearly closed and bloodshot, and his arms, thighs, and legs, especially the latter, in a raw and irritable condition, owing to his frequent scratching them during his sleep, though his legs were invariably protected by pillows, and his nails deprived of their sharpness by having gloves upon his hands. Besides this, he was extremely peevish in temper, necessitating his being dressed by main force, so intense was the agony produced by every attempt at exposing the diseased surfaces. His asthmatic attacks were also extremely frequent, and complicated with enlarged tonsils, chronic bronchitis, and a distressing cough.

My first efforts being, as often happens in private practice, to relieve symptoms, I ordered two grains of the iodide of potassium three times a day, as well as a mixture containing sulphuric ether, small doses of ipecacuan and antimonial wine internally, and oxide of zinc ointment, washed lard, and light bread poultices externally, according to the state of the parts, whether dry and scaly, excoriated, or covered with scabs.

The good effects produced by the zinc ointment were well marked when properly applied for a few days, but I soon found that a better effect was produced by using ʒij. of the oxide of zinc to the ounce of lard. This treatment having been persevered with for a fortnight, I resolved to try the alkaline-lotion treatment, which was used as follows:—Linen rags saturated in a solution of carbonate of soda, ℥ss. to the pint of water, were kept constantly moist by being soaked in the lotion several times a day, and covered over with oil-silk, the zinc ointment being still applied to the face; but it gave rise to such intolerable itching and irritation that, after a week's trial, I was obliged to abandon it, having found it impossible to prevent his tearing off the dressings in his sleep, which, of course, was followed by great restlessness, intense pain, and an angry-looking and bleeding surface. The washed lard was again resorted to, which, if it did nothing else, effectually prevented extraneous particles adhering to and irritating the diseased surfaces, and helped to induce a general tranquillity that had not been experienced for a long time.

Having gained this much, I took advantage of an opportunity for applying equal parts of the ung. picis liquid. and sapo mollis to the driest parts, with the greatest possible advantage; and, indeed, I believe that this agent is equal to, if not superior to, most local remedies in very chronic cases. A few weeks of the above treatment worked such a wonderful change that there now could be no more doubt a speedy cure was on the eve of being accomplished; but as the face and arms began to heal, his friends imagined that the asthmatic paroxysms became more frequent and severe—an opinion in which I did not feel myself quite warranted in acquiescing, although the coincidence was somewhat remarkable, considering that it was now midsummer, and that every precaution had been adopted to prevent his catching cold.

The bromide of potassium was substituted for a time instead of the iodide, but I cannot say that it produced any marked result; and the cautious inhalation of the nitrate of amyle was also tried on two occasions, but, in justice to this remedy, it did not have a fair trial, as the attacks had nearly passed off before I had an opportunity of using it.

Towards the end of autumn I was glad to find that the greatest possible change had taken place. The face and extremities had completely healed, leaving only a few isolated spots here and there, which soon disappeared after a few applications of the pitch ointment.

The asthmatic attacks also became less frequent, and there wa a great improvement of his general health.

At the close of the year I pronounced him cured, so far as th skin disease was concerned; and at the date I am writing this paper (December 1873), his mother writes me that the cure has been completed, that occasional small spots reappeared last January, but were soon dispelled by a few applications of the "dark ointment," and that, with the exception of a wheezing respiration, he is as lively in every respect as any boy of his age ought to be.

Remarks.—As the pathology of eczema is at present in a rather unsatisfactory state, I need only observe that I agree with those who hold that it is essentially a vesicular disease, so far as can be ascertained from its external characteristics in the early stages; but I would go further, and say that the eruption is simply a manifestation of some unknown internal cause being at work.

In this case, the cause would seem to have been internal, though there was nothing to account for it, unless the mother having undergone some fatigue during pregnancy, could have acted as a predisposing cause; and as the child was suckled by herself, it simply remains a matter of conjecture how far maternal influences might have been at work. As regards the treatment adopted, a casual reader might imagine that the administration of arsenic, which is a favourite remedy with many, might have expedited the cure. Perhaps it might, but that does not detract from the simple and efficient means that were employed. In private practice one can scarcely afford to waste time upon popular remedies, the actions of which are more or less doubtful, but rather to adhere to those which are most likely to ensure success, as this is the only means by which we can attain public confidence, and the lasting gratitude of helpless sufferers.

ARTICLE IV.—*Experiments Confirming Dr Fergus's Views as to the Passage of Gases through Water-Traps* (see last month's Journal). By JAMES M'TEAR, F.C.S., etc.

(*Read before the Glasgow Philosophical Society.*)

THE experiments shown here at our last meeting by Dr Fergus on the ease with which sewer-gases would pass trapped pipes, were most interesting; but I am afraid, from remarks made by various gentlemen, both at the meeting and afterwards, that they were not sufficiently conclusive. It was said at once, these traps are not ventilated, and so you cannot expect any other result than that the gases will pass, more especially as they are under pressure. Now, in the first place, very few traps are ventilated (although, no doubt, they should be), and in most cases we have actually a pressure in the soil-pipes. It occurred to me, after seeing at last meeting the experiments, that, as it was evidently owing to diffusion that the gases passed, no trap would keep them back, even if it was ventilated, given a sufficient length of time; and I had a series of experiments made, which I believe to be quite conclusive on this point.

The first series was made in glass tubes; and it having been suggested to me that it would be much more convincing to unscientific minds were I to repeat them with actual lead traps, I had another series carried out with the traps before you.

The usual size of a ventilating pipe on a 3 or 4 inch trap is from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch, being in the ratio of from $\frac{1}{64}$ to $\frac{1}{16}$. In first series experiments it was from $\frac{1}{16}$ to $\frac{1}{12}$ nearly. In second series of experiments it was from $\frac{1}{3}$ to $\frac{1}{15}$. In all of them much larger in proportion than is usual when ventilation is applied, which, however, is rarely the case.

A number of experiments were made and various gases were used, the last tried being coal-gas, which bears some resemblance, in general composition, to sewer-gas. Besides these, putrid beef-juice, egg, and urine were used, with the hope of finding that the minute organisms seen in these decomposing substances would pass through the trap. In this, so far, I have been unsuccessful; but I quite believe that they will so pass through, if time and favourable conditions are given. The odour of putrid matter passes most readily through these traps, and is quite distinct to the sense of smell.

I have here some diagrams which show the passage of the gases through the traps. A curious fact, and yet one quite to be expected, is here shown; viz., that light gases pass through by the top of the bends, and heavy gases at the bottom; also, that the gas does not saturate the water in the trap by any means, but that it first saturates the surface, next the vapour, then the gas seems to sink down in a fine stream and gradually travels through the liquid to the other side, when it again spreads out and begins to diffuse, both into the atmosphere above it, and downwards through the water in the trap; in fact, the most apt illustration of the general appearance, is the gradual dispersion of a so-called smoke ring from the funnel of a locomotive.

1st. I have, in first place, diagram showing passage of NH_3 (ammonia). This in glass tube, ventilated as you see, showed on surface in 1″, began to move downwards in 5″, reached surface at other side of trap in 15″, and rendered litmus paper, suspended in tube, blue in 20″. With the wide tube open, it began in 1″, and coloured litmus paper in 100″.

2d. Cl (chlorine), diagram shown, began to show in 2″, completely through in 2 hours; with wide tube in 2½ hours. The movement in the case of Cl and other heavy gases is comparatively rapid while passing downwards, but very slow in passing upwards in the other limb (Indigo solution in water).

3d. Co_2 (carbonic acid) passed through in 60″ with small tube.
" " " 80″ " wide "
(with litmus.)

4th. H_2S (sulphuretted hydrogen) passed through in 80″ with wide tube.
" " " 100″ " small tube.

This experiment showed that it is not necessary that the liquid should be saturated, as it took 2½ hours after the gas had passed through to destroy the remaining blue colour in iodide of starch.

5th. Coal-gas, with small tube, through in 2 hours.

We had to depend on the small amount of sulphur compound present to help us to trace the movement in this case through solution of iodide of starch, and the reaction is very faint, but the smell was most distinct.

Second Series.

4 inch large lead trap, ventilating tube 1 in. dia., 2 ft. long.
1st. NH_3 passed through 3 inches of water in 3′ 20″.
2d. Coal-gas ,, ,, ,, 4′ 35″.
3d. Putrid egg and urine; smell passed through in 3 days.

Third Series.

Small lead trap 3 in. dia.; ventilating tube 1 in. dia., 2 ft. long.
1st. NH_3 passed through in 2′ 55″; 2 inches water in trap.
2d. Coal-gas ,, ,, 1′ 25″ ,, ,,
3d. Putrid beef-juice; smell through in 3 days.

I submit, Gentlemen, that these facts prove conclusively that, ventilation or no ventilation, gases do pass through, if we only give them time; and it is quite certain, that if we were to extend these experiments further, we would find that after the water in the trap has become saturated, the rate at which the gas would pass would be greatly increased, for we have in nearly all cases a considerable difference in the temperature of the outside sewer-pipe and the house, conducing greatly in favour of the flow of gases inwards.

I think most of you will admit that these sewer-gases will and do pass into our houses; and it seems to me that we cannot, in the face of such evidence as has been laid before us from time to time, lay the flattering unction to our soul, that these gases are quite harmless. No doubt you have heard it said, as I have, about our friend Dr Fergus, that he had sewer-gas on the brain; but, Gentlemen, if this sewer-gas mania (if you like to call it so) should lead to the saving of even *one* human life, should we not have cause for thankfulness? I think that we should, before the close of this discussion, ascertain the feeling of the Society on the question of danger, or no danger, from sewer-gas.

As to ventilating pipes, merely one word in closing. In ventilating any space, it is necessary to have a supply of fresh air as well as to remove the foul. In trapped pipes we have no provision for this, and so our ventilating pipe acts mainly by diffusion. If, instead of one, we apply two pipes, one a good deal higher than the other, we may then do some good—the fresh outside heavier air descending the one tube, and the impure lighter air ascending the other; but I am thoroughly convinced, from the experiments I have described, that no ventilation will altogether remove the danger of sewer-gas getting into our houses.

I may mention that in these experiments, the gases were not given off under pressure, and that the large trap contains, of water, 228 cubic inches or 132 oz., and the small trap, 45 cubic inches or 27 oz.

ARTICLE V.—CHILDREN'S HOSPITAL PAPERS. No. I.—*Clinical Observations on Pneumonia.* By WILLIAM STEPHENSON, M.D., F.R.C.S. Ed.

(*Continued from page* 628.)

THE treatment adopted in the cases recorded, except where otherwise stated, consisted of warm poultices to the chest, and a simple mixture containing acetate of ammonia, so that they may be regarded as having run their natural course uninfluenced by medicine; my object being to first establish a basis as to the natural history of the disease in its different phases, for future comparison, and the obtaining of some principles to regulate the treatment.

The physical signs of pneumonia, we have seen, cannot form alone the basis of classification, nor determine the treatment. The whole clinical chart must be taken into consideration.

Nor can an aggregate mortality—too often taken as the criterion—ever fairly represent the result of therapeutic measures. For in so testing the subject there are included the cases which would have proved favourable, whatever the treatment, and those also which would be fatal, in spite of all that could be done. The physician's art is one of specialization, and any definite result cannot be obtained by applying to it the principle of generalization, however useful that method may be in its legitimate sphere. A carefully particularized study of one case, accurately and fully recorded, may be more fruitful in practical results than fifty or a hundred thrown loosely together.

Treatment may be directed towards very different ends, and it is part of the physician's art to determine for what purpose his aid is required. Many cases occur where nothing further is necessary than to place the patient under the most favourable circumstances for recovery. Some remedial measures are required in others, not as against the disease, but to relieve the suffering and promote the comfort of the patient. At times, also, concurrent phenomena may arise, endangering life, distinct from the phenomena proper to the disease, and these must be met. While, lastly, the treatment may be directed against the disease proper, with the aim of modifying or cutting short its course.

Nothing I know of in medicine entitles us to expect that we can ever change for good the well-marked typical course of acute pneumonia. Before ever the patient can be brought under our care, the system has received a morbid impulse, by which a pyretic wave has arisen, which will run its course. There can be no true cutting short the disease. Where such a term is used, it can only apply to the prolongation out of due limit of the affection. Any time after the fifth day a rapid and favourable termination may occur, and this natural course has undoubtedly led, in former times, to very erroneous opinions as to the result of active interference.

The test of treatment should be the closeness with which the fever accords to the normal type. It is not necessary to show any material lowering of temperature or pulse, so long as these are within the normal range; but it is essential, whether the range is affected or not, that the typical course should be retained. Should the treatment adopted succeed in lowering the range during the first five or six days, but at the same time the chart show a divergence from the normal character as regards crisis, and especially if the stage of convalescence is delayed, then I should regard such treatment as unsuccessful and meddlesome, although it terminated in recovery.[1]

When the range of temperature or pulse is above the normal for the disease, or the one out of due relation to the other, such excess should be combated. And if evidence of reduction is found, it is then legitimate to gain it at the expense of protracting the recovery. If, again, life be endangered by any special condition, the means of counteracting it may likewise be bought at the expense of the convalescence. But, with these exceptions, the value of therapeutic agencies is only to be estimated by the accordance with the typical character of the disease.

The alchymists of old were great experimentalists, and succeeded in forming many chemical substances, but the science of chemistry made no advance so long as air and water were regarded as elements, and that phlogiston entered into and devoured a body like a ravenous animal. Nor is our position far different so long as we regard and treat fever and inflammation as either simple or invariable compound states, or that there is any entity in disease, to be met by the same treatment in all cases. That fever is a complex state, and that its elements enter into varying proportion with one another, is duly recognised in the science, but much less so in the art of medicine. We have two of its elements, and are able to estimate and record them in the temperature and pulse, and these have of late been too much regarded as the sole representatives of fever. But there is another element which we can, as yet, but inferentially estimate and cannot record, and on this account, probably, it has been too much lost sight of.

Physicians, in feeling the pulse, constantly take cognisance of more than its rapidity; the character is all-important. We constantly see the same phenomena, especially nervous symptoms, occurring under two very different and opposite states of the system. The same temperature may occur at the beginning and at the end of a fever, but with different significance. Why is the diurnal range of temperature at the beginning of a fever so much more steady than at the end? Why are some febrile states characterized by great oscillations between morning and evening temperature? There is an important something in all these which is not directly represented in our charts, but of which, in treatment, we must take cognisance. This something I am in the habit of speaking of as tension—a term used in electricity, and probably a closely allied condition to what it there represents.

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[1] This, according to M. Barthez, is the effect produced by indiscriminate active measures, especially bloodletting.—*Syd. Society Year-Book*, 1862.

The estimation of this condition is of great importance. The degree of tension makes up the difference between two cases which may present the same average temperature. Treatment may be directed towards it, and we may succeed in lowering the tension without affecting the temperature: another point which is too much overlooked at the present time, when temperature is taken as the chief representative of fever. The relative range of temperature and pulse, I am inclined to think, may yet afford us some means of estimating the tension. Thus, in the case of Lonie (No. 3), there occurred the temperature of 106°, with a pulse relatively much too low, viz., 124, and the tension at the time was very high, while in other cases 104° was associated with a pulse of 130 to 136; and this is just the condition which physiology teaches us would exist. On the other hand, as we have seen, the fatal cases presented a pulse relatively too high for the temperature. If I am right in my inference, successful treatment may be represented by an actual rise in the rate of the pulse.

The influence of tension on temperature may also perhaps be seen in the degree of oscillation between morning and evening, where tension is great, as at the beginning of the inflammatory attack the range is even, but when slight the oscillations increase, and when they have been well marked, and a complication occurs, as in the case with empyema, the temperature again becomes steady.

Thus we may be able inferentially to deduce the amount of tension from the data on our charts; and the evidence so gained must be carefully weighed in deducing the results of treatment. In the treatment of acute febrile diseases, the attention of the profession has lately been entirely directed to the reduction of the pulse and temperature, and the value of remedial measures estimated by this alone. By so doing, it is my opinion that we lose sight of many important indications, and are apt to overlook much benefit that may be derived from remedies which do not affect directly the temperature. If the height of the temperature is within the normal type of the disease, and if the pulse is within the normal relation to that temperature, little good will be got by any attempts to alter their range, etc.

As I have already pointed out, the prognosis of pneumonia depends much less upon the height of the temperature alone than upon the relation of pulse and temperature. It is therefore to this point our attention should be addressed, and to the other means whereby what I have termed tension may be estimated.

The use of warm compresses to the chest is of great value in all cases. Where tension is great, and where head symptoms are present, the whole body may be placed in a warm "pack." No means is more conducive to the comfort of the patient; the general uneasiness and sense of fulness of the system is allayed, and natural sleep induced. At the same time, it must be remarked, I have never observed any decided change in the temperature or pulse from the use of the pack, but nevertheless the quiet and comfort afforded thereby is very evident.

Where further means are necessary to reduce tension, we have the selection of three valuable remedies—aconite, antimony, or ipecacuan; but the good to be got from these is only within the first thirty-six or forty-eight hours. They should not be continued longer, and never, for any theoretical action upon the lung lesion, should they be given when the pulse is relatively high for the temperature. The large majority of cases, indeed, may be best treated by ipecacuan or aconite, and acetate of ammonia. The value of aconite in acute inflammatory disease may be denied if we judge of it by its power of lowering pulse and temperature, or cutting short the disease. Looking for its action in these quarters, I long disbelieved in its good effects, but have lately had my confidence in it renewed by observing the increased comfort of the patient, and the lowering of the tension where high, under its use. I have not yet, however, been able to record a definite action in the clinical chart to change mere opinion into conviction. In case 5 (John Gill), where aconite was given throughout the febrile attack, it is worthy of note how the convalescence was prolonged by recurring high temperatures. Without assigning this to the effect of the unnecessarily prolonged use of the remedy, the circumstance must be noted at present in connexion with it.

In a few rare cases, in healthy children, where the onset of the disease is associated with considerable dyspnœa, bloodletting by leeches between the scapulæ may be employed with undoubted effect; also, when pain in the chest is very severe and preventing rest, a single leech to the seat of pain will afford great relief.

Where the pulse is high in relation to the temperature, quite an opposite line of treatment must be followed. Here quinine, iron, and digitalis, singly or in varying combination, are the remedies indicated. In pneumonia occurring amidst the sequelæ of scarlet fever, I have seen good results also from belladonna. The selection must turn upon the due recognition of the constitutional state giving rise to the inordinate pulse. Various conditions may so act—the effect of the high temperature upon the nervous system, the amount of blood deterioration, a general cachetic state, or simple atony of the muscular vascular system.

Several remedies may likewise be employed to minister to the comfort of the patient. I would specially mention morphia and spirit of chloroform.

Salines may be given from the first, in combination with the other medicines enumerated, and their use may be continued with benefit after the febrile stage has passed off—during the process of resolution. In this stage alone, and when the health of the child has been previously good, is any benefit ever to be derived from the use of mercurials. Where the process of resolution is tardy, or has been arrested, I have seen decided benefit from *small* doses of mercury.

The space at my disposal permits only of thus indicating the principles upon which the treatment must be regulated. I would only further remark, that the value of such treatment must be estimated by these principles, the soundness of the evidence upon which they are based, and the result of their application in *individual* cases; whilst the skill of the physician is displayed in the proper estimation of the "situation," in the foresight of danger, and in the practical working out, in each individual case, of the principles which his science has established.

ARTICLE VI.—*Report of a Case of Intracranial Tumour; with Remarks.* By S. LAWRENCE, M.D., Montrose.

(Read before the Medico-Chirurgical Society of Edinburgh, 4th February.)

BRAIN diseases are proverbially protean in their symptoms, and uncertain in their seat and duration. How frequently does it happen that the diagnosis and prognosis formed regarding them, plausible and well-founded as they appeared at the time, are utterly falsified by the result—that post-mortem appearances are met with which were not in the reckoning, and that others which were confidently expected are nowhere to be seen! Perhaps in no class of diseases does each individual case so distinctly claim to possess features of its own, both in respect of its clinical history and the morbid phenomena which autopsy reveals. If these remarks hold true of apoplectic and inflammatory affections, and lesions of the brain resulting from injury, they are not less applicable to intracranial tumours, or adventitious formations, of whatever kind, developed within the skull. Such tumours present very great diversity, not only in respect to their pathology and seat, but their entire clinical history; that history comprising their probable origin, the phenomena which have marked their progress, the period over which they have extended, and the symptoms which have preceded and ushered in the fatal close. In regard to their pathology, perhaps no two systematic writers will be found to adopt precisely the same classification or arrangement. While Dr Abercrombie,[1] for instance, reduces all organic cerebral diseases to eight divisions, and Niemeyer[1] makes them amount to eleven, Drs Reynolds and Bastian[2] enumerate not fewer than nineteen different kinds of adventitious formations, or deposits, which may be met with in the brain. Then, as respects the clinical history of such diseases, it would seem to be no easy task to effect any satisfactory and unexceptional arrangement of them, resting on this basis. Dr Abercrombie, however, makes the attempt, and specifies seven distinct classes, according as certain symptoms, or groups of symptoms, are prominent during the progress of the case. There is still another arrangement of such intracranial diseases which might be aimed at, based on the relation which their symptomatology holds to their diagnosis; in other words, according to the varying degrees of plausibility and accuracy, or, *vice versa*, of uncertainty and obscurity, with which, by means of local and constitutional symptoms, we are able to form our opinions regarding them. Proceeding upon this principle, four groups might be made to comprehend all varieties. 1*st*, Intracranial tumours are not unfrequently met with, presenting such marked special and general symptoms as to indicate not only their existence, but also their seat. 2*d*, In another group, while the local and constitutional ailments point strongly to the existence of such tumours, they throw little or no light on their special location. 3*d*, In a third group the symptoms are altogether misleading, withdrawing the attention of the physician from the brain, and fixing it on some distant organ, say the stomach or liver, in his search for the *fons et origo* of the morbid phenomena. And, 4*thly*, In a certain number of cases the adventitious formation is altogether latent, having lain for months, perhaps for years, embedded in the brain, without giving any outward sign, direct or indirect, of its existence, and is at length only brought to light by the scalpel of the pathologist, after the subject of it has died of some other and quite unconnected disease.

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[1] Pathological and Practical Researches on Diseases of the Brain and Spinal Cord, by John Abercrombie, M.D.

It may be remarked, in connexion with the first of these four groups, that Niemeyer has ingeniously arranged some seven or eight separate sets or combinations of symptoms, by which he holds it is possible to differentiate as many distinct locations which cerebral tumours may occupy. But he follows up his interesting detail by the just observation, that, "even on most carefully weighing all the symptoms, many errors may occur." It may be presumed that few will hesitate to endorse such a candid confession; for, arrange and classify as we may, individual cases will perpetually cross and recross all our artificial lines of distinction; and our experience and our beliefs will be thrown into new and ofttimes very different channels from what our previous study or imagination had prepared for them.

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[1] A Text-Book of Practical Medicine, by Dr Felix von Niemeyer. Translated from the German by Dr George H. Humphreys and Dr Charles E. Hackley, New York.

[2] A System of Medicine, edited by Dr J. Russell Reynolds, vol. ii.

I now proceed to relate a well-marked case of intracranial tumour, which doubtless had much in common with all such morbid productions, but which, at the same time, possessed such pronounced individuality as apparently to justify its detailed narration. James C., æt. 21, up to the time his illness commenced, was in the enjoyment of good health. He was tall—upwards of six feet—and of rather slender make, of regular habits, and of a pleasant, cheerful disposition. He was occupied in assisting his father, who carried on the business of a house-painter, and spent a considerable portion of his time in book-keeping and writing accounts. Without any premonitory symptoms, or any apparent cause, he was seized, during the night between the 15th and 16th December 1868, with violent headache and vomiting, followed by incoherence, and then by more or less stupor. His pulse kept low, ranging from 54 to 60, and was labouring, but steady. Leeches were applied to the temples, ice over the head, and small doses of tinct. aconit., frequently repeated, were administered; a free calomel purge was also given. Attention was also directed to the state of the kidneys, which acted freely. By the 19th there was less stupor and pain in the head, but strabismus had become developed; the pupils were more contracted than natural, but were sensitive to light. During the next ten days there was, on the whole, a subsidence of the more serious symptoms. He was enjoying natural sleep, taking a fair amount of nourishment, complaining very little of pain in the head, and his pulse had now risen to between 70 and 80. On the 30th the entry is, "Improved in every respect, excepting *squint*, which continues."

From this time convalescence progressed satisfactorily, being only once interrupted for a day or two by an attack of vomiting, which occurred about the middle of the month. The strabismus persisted for several weeks, and was the last special symptom to disappear. By the beginning of January I was able considerably to reduce the frequency of my visits, and by the 10th February to cease my attendance on the case. An illness of a most serious and alarming nature had thus been got entirely the better of. Not the smallest vestige of a head symptom remained, and any general weakness which was left behind was gradually recovered from by rest, nourishment, and country air, when the proper season came round. In due time he resumed his business, and during the latter part of 1869, and throughout 1870, he conducted a branch of his father's business in a village ten miles distant, continuing in the enjoyment of perfect health. I seldom saw him during that period, but was frequently hearing of him from his family, and the report was uniformly of the most satisfactory kind. At the end of the first week of January 1871, I had the opportunity of subjecting him to a careful examination on the occasion of his wishing to effect a life assurance; but the utmost scrutiny failed to detect any trace whatever of the dangerous illness through which he had passed upwards of two years before, or any flaw in his general condition. Throughout January and February he continued well, and actively engaged at business. On the 14th March (that is, 1871) I was asked again to visit him, and learned that for the fortnight preceding he had had occasional attacks of pain in the head, followed by slight giddiness. Some alterative medicine was ordered; and as he returned to business in the country, I did not see him again till toward the end of the month. I then found him still complaining of pain in the head, with more or less oppression, foul tongue, and a little excitement of the pulse. Leeching, free purging, and perfect quietude, soon brought much relief, and by the 5th April convalescence seemed to have begun. By the 7th, however, he was again complaining of giddiness and pain in the head, which was also frequently seated in the back of the neck. His pulse was 90; his tongue foul. During the rest of April his condition varied considerably, the headache and giddiness[1] being at times almost entirely gone, and at other times returning with considerable severity. Early in May he had greatly improved. I saw him seldom during that month. I visited him on the 28th, and found him as follows:—"Pain in the head has now ceased, but giddiness and dimness of sight are troublesome. Sleeps well, has a fair appetite, tongue rather foul, and bowels are inclined to constipation."

It will be observed that a new symptom had now, for the first time, shown itself, viz., dimness of vision, which went on with deepening intensity, becoming ultimately of profound significance. The last visit I paid him, before he removed to the country for the summer months, was on the 12th June. At that time he was continuing free from headache, but still had more or less giddiness and dimness of sight, confined to left eye. The pupils of both eyes were a little sluggish. The appetite was fairly good. The strength had rather improved. The pulse inclined to be somewhat excited.

I did not see him again till the 30th September, when he had returned home. He then expressed himself as free from pain and giddiness. He could now with difficulty discern any objects with the left eye, and the sight of the right one had for some time been gradually becoming impaired. His lower extremities also had lost somewhat of their former power, so that his gait had become a little unsteady. He ate and slept well; his intelligence was intact; and notwithstanding the sad, hopeless character which his case was now fast assuming, he was contented, and even cheerful. On the 10th October he was continuing free from giddiness, but had more

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[1] What is the precise *nexus* between "giddiness" and organic disease of the brain, incipient or developed, we can only conjecture, seeing we are yet much in the dark as to the physical or proximate cause of that peculiar sensation. We smile at the crude fancy of Paulus Ægineta, that "vertigo is occasioned by a cold and viscid humour seizing upon the brain," but are we able, after the lapse of nearly thirteen centuries, to replace such error with truth?

or less pain daily in the back of the head. The sight of the left eye was now entirely gone, and that of the right one was very dim. His limbs were getting weaker, but otherwise his general health was very good. He was now for a time put on bromide of potassium, twenty grains night and morning, to which, by-and-by, a little liquor strychniæ was added. His bowels were sluggish, and required the occasional stimulus of purgatives. The symptoms presented but little change up to the end of the month. On the 16th, he had a slight attack of syncope, which, however, soon passed away. The pain in the head varied considerably as to degree, being at times slight, and at other times severe. At two separate visits—viz., on the 18th and 23d—he assured me that his sight was improving. This was probably imaginary, for by the 1st November the entry is, "There is now almost total blindness." At this distance of time, I am unable to say whether the probability of organic cerebral disease—in other words, of the development of a tumour—as the cause of the symptoms, suggested itself to my mind before the 28th May, when incipient dimness of vision was noted; but from that time onward this was my growing belief, and long before J. C.'s death, my assured conviction. The slow but progressive failure of sight, first in the one eye, then in the other (taken in connexion with all the other symptoms), and culminating at length in total blindness, seemed to leave no reasonable ground of doubt that organic disease was at work at the base of the brain, involving the optic thalami or commissures, so that paradoxically it might be affirmed that the *increasing darkness* of the patient only threw *increasing light* upon his melancholy case. The condition of the heart, as well as of the kidneys, was again and again inquired into; but no deviation from the normal state of either of these organs was found. On the 7th November I had a consultation with my friend Dr Howden regarding this patient. He examined him most carefully, and corroborated the opinion at which I had arrived as to the extreme probability of tumour at the base of the brain. Besides the weakness of the lower extremities, there was now slight diminution of power in the left arm and hand. This, however, never increased. At Dr Howden's suggestion, ten grains iodide of potassium were ordered to be given night and morning, and this was continued for some months without any apparent benefit resulting from it. From this time on to the end of December there was little or no change of symptoms. Some days were passed almost free of giddiness and pain; at other times both feelings were experienced, the pain being generally referred to the vertex and lower part of the occiput. He was now more closely confined to bed than formerly, finding *dorsal decubitus* the position that gave him the greatest immunity from attacks of pain. He became extremely sensitive to noises, which were always followed by more or less pain. In the condition described he passed out of 1871 into 1872; his general health was remarkably good, his relish for food, and his enjoyment of sleep, being almost equal to what they had been in former days.

1872.—January passed without any change. On the 17th February, he had an attack of vomiting, which recurred the following night; and on the 22d, after an exacerbation of pain in the head, he was seized with an epileptiform fit, attended by slight and temporary unconsciousness. On the 23d, he had an attack of vomiting; and on the day following, a severe paroxysm of pain in the head, which at this time showed a great tendency to recur on lifting the head even for the shortest time from the pillow. By the end of the month he seemed again back to the point at which he was at its commencement. Twice in March—viz., on the 11th and 26th—he had a recurrence of epileptiform seizure, on both occasions while, for a short time, in the sitting posture, and after some slight exertion. The attack seemed to leave no permanent bad effects behind.[1] There was nothing special to note during April, excepting occasional paroxysms of pain in the head, and increasing weakness of the lower extremities. On the 1st May, I drew up a history of the case, and forwarded it to Dr Begbie, Edinburgh, for his opinion. In the letter which I received a few days after in reply, Dr Begbie said, "I entirely agree with you in regarding your patient as the subject in all probability of cerebral tumour, and certainly of profound intracranial disease. Your opinion as to the likely seat of the disease admits, I think, of little question." . . . "I repeat my full belief in the existence of grave disease at the base of the brain, the disease being more likely tumour than either abscess or inflammatory product." He recommended a trial of liquor hydrargyri bichloridi, in teaspoonful doses, twice daily, but added, "While suggesting this, I agree with you that the general treatment of such a case is best directed to the promotion of the patient's comfort."

From this time onward to the end of October, there was no marked change in the case. He was for days, and sometimes even for weeks, almost entirely free from attacks of pain, ate and slept well, and, although totally blind, was cheerful and happy. His pupils were largely dilated, and insensible to light; his limbs became so feeble that he was quite unable to support himself, and on this account, and in consequence of the tendency to a recurrence of the headache in assuming the upright position, he was by the end of September seldom out of bed. About the middle of November he had a severe paroxysm of pain in the head, accompanied with vomiting, excited by continued loud knocking in the adjoining house. Throughout December his condition was uniform, only it was now remarked that any attempt to raise the head from the pillow excited more or less unconsciousness as well as pain.

1873.—January, February, and March, passed away without any obvious change in his state. He had continued closely confined to bed, owing to the tendency to pain and unconsciousness above referred to on the head being moved from the recumbent posture; he further complained of his inability to support his head when it was lifted from the pillow. The power of his limbs was now all but entirely lost, but there was no anæsthesia. His general health seemed unimpaired, his intellect untouched; his spirits, notwithstanding his sad and hopeless condition, as buoyant as aforetime. He was happily situated in respect to his outward and family circumstances, enjoying every comfort, and receiving from every member of his domestic circle the most unwearied and devoted attention, for which he reciprocated the liveliest gratitude. In the affairs of the outer world he continued to take a lively interest; and a stranger hearing his observations on passing events, clearly expressed, and in a firm, unfaltering voice, would have felt it hard to believe that he was the doomed victim of serious cerebral disease. Such was the state of matters up to the 12th April, there being nothing in his condition on the morning of that day more than there had been for many months before, to portend that a fatal change was at hand. The forenoon passed as usual, and he partook of an early dinner with his accustomed relish. Between four and five o'clock P.M., without any previous warning, he was seized with severe pain in the head, followed by vomiting. Having witnessed similar attacks so frequently before, those about him regarded the present one without any special apprehension or alarm. His mother applied cold to the head and heat to the lower extremities, and by-and-by a measure of relief was experienced. He then lay calm, was quite conscious, and spoke of trying to take a sleep. His mother left him for about five minutes, under the impression that the desired sleep would soon be enjoyed. When she re-entered his room and approached his bed, she found him with both hands placed over the top of his head, and looking as if he had sunk into the most placid slumber; but it was soon too evident that breathing had ceased, and that the sleep into which he had fallen was the sleep of death.

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[1] I did not happen to see him during any of these epileptiform seizures, but from the account given they seem to have been slight and transient.

An examination of the body, at which I was present, was made by Dr Howden, on Monday the 14th, and he afterwards kindly furnished me with the following report:—

"*Post-mortem Examination.*—Scalp normal. The calvarium was remarkably thin, and translucent throughout. When it was removed, the brain bulged out as if from pressure within. On manipulation it fluctuated like a bag of fluid. The convolutions were flattened, and the spaces of the sulci obliterated. The membranes were thin but normal. On opening the lateral ventricles, it was found that they were greatly distended with a clear serous fluid, of which twelve ounces were measured. The lining membrane of the ventricles presented a rough aspect from the presence of crystalline granulations. The nerve substance seemed normal, but on removing the brain a loose gelatinous tumour was found to occupy the base. From its loose, semi-fluid consistence, it was impossible to define exactly the boundaries of this body, but it occupied the fourth ventricle, and extended in front of the pons Varolii, spreading as far forward as the optic commissure, under the arachnoid. In the substance of the gelatinous mass in the fourth ventricle there was a small recent clot, about the size of a pea. The substance of the tumour, examined under the microscope, exhibited small, round, nucleated cells, about the size of white-blood-corpuscles.

"The thinness of the calvarium was, I believe, due to absorption from pressure of the fluid in the ventricles. Fluid in the ventricles is commonly associated with tumours at the base of the brain. Blindness was no doubt due to pressure of the tumour on the optic commissure and nerves. Death probably resulted from the small apoplectic clot in the fourth ventricle.

"JAMES C. HOWDEN."

I would now notice, with all brevity, what seem to be the more interesting points in the foregoing narrative.

First, As to the nature of the primary illness in November 1868. Was it apoplectic or inflammatory? The suddenness of the seizure, and several of the symptoms, imparted to the attack at the time of its occurrence much the appearance of a form of apoplexy, and I had difficulty in dispossessing my mind of the belief that sanguineous extravasation into some part of the brain had taken place. But the amount of pain endured, the absence of any approach to paralysis, and of any well-marked coma, the subsequent development of strabismus, and the progressive and comparatively speedy subsidence of the more serious symptoms under the employment of antiphlogistic remedies, combined, I think, to demonstrate that the attack was one of sudden meningitis. In its general outline and more salient features, it may be seen to present a close resemblance to what Dr Abercrombie described as "a fifth form of inflammatory affection of the brain," and which he viewed as a highly dangerous and too often fatal disease.

Secondly, The relation between the first and second illness. It is difficult to divest the mind of the suspicion that a *causal* relation did to some extent exist between the primary illness which occurred in November 1868 and the subsequent and ultimately fatal one, which, commencing in March 1871, ended in April 1873. No doubt the first attack seemed thoroughly recovered from; not a trace of head affection was left behind, and the general health was completely re-established, and continued unexceptionally good for a period of nearly two years. All this apparently militates against the view now suggested, and would seem to establish the independent character of the second illness. But considering how prone any part once the seat of inflammatory action is to subsequent structural and disorganizing change—how, *a fortiori*, this must hold true of an organ of such delicate texture as the human brain—how obscure is the pathogenesis of morbid growths—and how certain it is that a period of latency, longer or shorter, must always precede the outward symptoms which their presence in any organ ultimately excites,—I think the opinion I have expressed has a strong basis of probability, if not of absolute truth.

Thirdly, Specialties in the Case.—During its course, and especially at its termination, the case now recorded presented various features admitting of comparison or contrast with those more usually met with in tumours of the brain.

(1.) *Pain.*—There were freqnently during the last year of his illness long periods of almost entire exemption from pain in the head, and often when present it was slight and transient. This cannot be said to be in accordance with ordinary experience. Drs Reynolds and Bastian, while speaking of the occasional exacerbations which pain undergoes, when the sufferings of the patient are almost intolerable, say that "it is rarely absent during the intervals of paroxysm." And Niemeyer remarks, "There is no cerebral disease where continuous, usually intense and severe, headache is so prominent a symptom, and there is no other disease where it is so often observed as in tumour of the brain." An appeal to published cases amply bears out these general statements.

(2.) *The Condition of General Sensation.*—Neither anæsthesia nor hyperæsthesia showed itself in any part of the cutaneous surface, nor (excepting the blindness) in any of the special senses during the course of the illness, as we find to have frequently occurred in recorded cases of tumour of the brain. Loss or diminution of sensation is not an infrequent accompaniment of paralysis of the extremities in such cases; and occasionally, but more rarely, there is an exaltation of sensibility. In an interesting case, recorded by Dr Alexander Wood in the *Edinburgh Medical Journal* for January 1860, there was not only hyperæsthesia of the organs of sight and hearing, manifested in a most remarkable manner, but also distressing neuralgia in one of the paralyzed limbs.

(3.) *Convulsions.*—Only thrice, during the progress of the case, did these occur,—on each occasion to such a slight extent as hardly to deserve the name. In this respect, there was also a deviation from very common experience. Niemeyer says,—"Epileptiform convulsions occur more frequently from tumours of the brain than from any other local disease of that organ,"—and the testimony of other writers is found to corroborate this opinion.[1]

(4.) *The Close of the Case.*—The circumstances attending the fatal termination were not only unusual for a case of brain tumour, but, in some respects, might be regarded as unique. Convulsions succeeded by coma, exacerbations of headache occurring with increased severity and frequency, and followed by progressive exhaustion, are, according to standard authorities, the ordinary precursors of dissolution in organic disease of the brain, so that for days or longer the fatal event is foreshadowed and looked for. In Dr Wood's case, for instance, there were, from the growing severity of the symptoms, at least five days' notice of impending death. In my own case, on the contrary, it might be said there was absolutely no notice whatever. Up to within two hours of death, my patient was as well as he had been for weeks or months before; and the paroxysm of headache and vomiting, through which he then passed, was certainly not more prolonged or severe than what he had often before experienced. When this paroxysm was over, he seemed to be regaining his former condition; and while in the full possession of consciousness, free from pain, and calmly inviting refreshing sleep, he suddenly expired.

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[1] Grisolle (Traité de Pathologie Interne), speaking of cancer of the brain, says, "Les convulsions sont un symptôme assez fréquent. . . . Sur quelques individus, les attaques convulsives sont annoncées par de la céphalalgie, avec tendance au coma, et par un ensemble de symptômes cérébraux excessivement graves dont la durée est de plusieurs heures, et quelquefois de plusieurs jours."

Fourthly, *The Diagnosis*.—This was verified by the post-mortem examination, perhaps as fully as the majority of such cases admit of. I claim no credit in respect to it. The dimness of sight, deepening into total blindness, seemed so pathognomonic of organic disease, involving the optic nerves or commissures, that I felt shut up to the opinion I had formed, in which opinion, as narrated, Dr Howden and Dr Begbie at once concurred. Bamberger (quoted by Niemeyer) regards diagnosis of tumours of the brain as, with few exceptions, rather a guess than a diagnosis; and the determination of their locality, in most cases, impossible. Perhaps he stands alone in such a view—and justly so, seeing that the evidence of the symptoms is frequently all but conclusive not only as to the existence of the disease, but its seat. An interesting and ingenious speculation of Niemeyer on this subject may here be referred to. He denies that disturbances of vision, even amounting to complete blindness, so frequent in tumours of the brain, are due to lesions of the optic thalami, or any part of the optic tract. He argues, that were this the case, the motor nerves of the eye, which are under about the same conditions as the optic nerve, would be paralyzed, which rarely happens. He therefore maintains that amaurosis caused by cerebral tumours, depends on venous congestion of the eye, and the resulting structural changes in the retina and optic nerve, and are due to compression of the cavernous sinus and obstructed escape of blood from the veins of the eye. This compression of the cavernous sinus is effected, he says, after the tumour has caused such an amount of effusion into the ventricles as to interfere with the openings of the venæ galeni into the straight sinus. I do not presume to pronounce an opinion on this plausible hypothesis.[1]

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[1] Professor Sanders, in a discussion on a case of Gliomatous Brain Tumour, brought before the Edinburgh Medico-Chirurgical Society (see Edinburgh Medical Journal, Sept. 1868) by Dr Lauchlan Aitken, espouses the

Fifthly, The Duration of the Case.—It will be observed, that the period over which the case extended, counting from the commencement of the second illness, was two years and about six weeks. Recorded cases of cerebral tumours present very great diversity in respect to their duration—running their course, according to Dr Abercrombie (dating from the first symptoms), sometimes in two or three months, but more usually lasting through one, two, or three years. Dr A. mentions one case which seems to have gone on for fifteen years,—a period, however, which must be regarded as of altogether exceptional length. In the majority of such cases, I apprehend that we are without any reliable data on which to base even a conjectural estimate of their duration. On the one hand, life may be lengthened out beyond all expectations, even when the sufferings are severe; and, on the other, when no such sufferings are endured, a sudden and unlooked-for fatal crisis may supervene (as happened in the case of J. C.). Had I been asked a week, or even a single day, before the death of my patient, how long he was still likely to live, I should, of course, have felt embarrassed by the question. But, considering the lengthened intermissions of pain he enjoyed—his natural relish for food and his nightly sound refreshing sleep—it would not have seemed rash to predict that at least many months were still in store for him. The issue, however, shows how unwise such an augury would have been, and may help to impress the important lesson, that in cases of intracranial tumour, all prognostication as to the *time* of the inevitably fatal termination should be persistently declined.[1]

Finally, The Post-Mortem Examination.—Perhaps next to the tumour the most notable morbid phenomenon brought to light by the necropsy, was the very large quantity of serous fluid in the ventricles. Dr Howden, an authority in such matters, assures me that 12 ounces of serum, the quantity measured, is an amount of fluid very rarely met with in those cavities. It seemed to reflect light upon one symptom of the case, which, for some time before death, had been very prominent, namely, the inability complained of by the patient to support the head when raised from the pillow, and the pain and giddiness which any such attempt invariably produced. That the mental functions should have remained unimpaired to the last, notwithstanding the pressure, day and night, perhaps for many months, of so large an amount of fluid against the cerebral convolutions, is surely an astonishing fact, and one full of interest

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theory of intra-ventricular effusion being, in certain cases of organic disease of the brain, the cause of the resulting blindness. Dr Argyll Robertson, on the other hand, ascribes its etiology to optic neuritis.

[1] Grisolle (work quoted) refers to the duration of cases of brain tumour, and the mode of their fatal termination, in the following words: "Enfin, après quelques mois, une ou plusieurs années de souffrance, ils succombent. Les uns meurent graduellement, d'autres sont emportés violemment dans un accès convulsif; enfin quelques-uns succombent à une complication cérébrale, ou à quelque maladie intercurrente étrangère à l'encephale."

to the psychologist, as well as to the physician. It may be held to demonstrate the power of the mind, as well as of the body, to adapt itself to circumstances! I conclude by quoting and homologating the words of Dr Alexander Wood, as my apology for so fully detailing the foregoing case—"Were a careful record," he says, "of the symptoms and dissection in all such cases to be preserved, the accumulation of such evidence could not fail ultimately to conduce to an improved understanding of the connexion which subsists between structure and function, in an organ which has so many multifarious offices to discharge as the human brain."

Part Second.

REVIEWS.

Typhoid Fever: its Nature, Mode of Spreading, and Prevention. By WILLIAM BUDD, M.D., F.R.S. London: Longmans, Green, and Co.: 1873. Pp. 193.

THIS work is handsomely printed, beautifully illustrated, and forcibly written. It concerns a fever which is pre-eminently fatal among the rich, and in these latter years has twice thrown its shadow on the throne itself; the first time, as we all too well remember, its victim fell, and a nation was plunged in woe. The second time a happier chance brought the unwonted sight of a whole nation prostrate before a higher throne, in grateful thankfulness for a life preserved; and yet, as Dr Budd very clearly shows, over 15,000 lives are yearly sacrificed by that nation's cruel and selfish indifference to the sufferings of those of less than royal rank. The king of Dahomey dies, and his affectionate but ignorant people chop the heads off a paltry hundred or two of his wives and best friends, that the poor man may not pass unaccompanied into the unknown spirit-land, and instantly loud outcries are heard as to the cruelty that dwells in the "dark places of the earth." And yet, in this most favoured nation, where the light of Christianity is regarded as having long since banished cruelty, over 15,000 lives are annually permitted to be swept away by one perfectly preventible plague, while over 140,000 annually pass through the protracted miseries of the disease—miseries which no one can realize who has not experienced them in his own home, and the full intensity of which no one can duly appreciate who has not also observed them in the cottages of the poor, who have but scant provision for the bare necessities of life, and none for its great emergencies. "How often," says Budd, "have I seen in past days, in the single narrow chamber of the day-labourer's cottage, the father in his coffin, the mother in her sick-bed in muttering delirium, and nothing to relieve the desolation of the children but the devotion of some poor neighbour, who, in too many cases, paid the penalty of her kindness by becoming herself the victim of the same disorder."

"In its ordinary course, however, life has few such consummations of misery as this." Yet, year by year, such misery is permitted to occur in a nation which boasts of its civilization and its Christianity, which pays its petty tithes of mint and cumin in church collections and infirmary subscriptions, and neglects and ignores the weightier matters of that law of Christian love which would render such misery impossible.

But, indeed, so-called medical science cannot itself be held to be irresponsible for much of this neglect, so long as such respectable works as Tanner's "Practice of Medicine," and Hooper's "Physician's Vade-Mecum," teach that the doctrine that typhoid fever is contagious and chiefly propagated by the excreta, is "an illusive hypothesis,"—just so long must the general public in a great measure stand absolved from the sad results which flow from such a belief. In this well-written work, Dr Budd gives clear and forcible proof that this belief is not an illusive hypothesis but an actual fact, which if duly and effectively acted upon, would go far to extinguish this fearful plague. It is true that his idea of the eruptive character of the fever is entirely fanciful. It is equally true that his scent of contagion is a little too fine, and that this flows from his opposition to the pythogenic theory of spontaneous development; but it is also true, and apparently incontestably so, that he has proved that the poison of enteric or typhoid fever multiplies its germs within the bodies of those affected by it, and that by means of these germs, contained within the excreta, it is chiefly propagated. We believe these to be facts, the truth of which admits of no doubt. We have long acted on a belief in their truth, and apparently with perfect success, in so far as the prevention of the spread of the disease is concerned; still, we also hold that there are many cases in which the spontaneous and pythogenic origin of typhoid fever is much more probable than any origin from a problematical contagion. The subject is a difficult one. About ten years ago, a friend was seized with typhoid in an isolated country house, and her disease was believed to be of purely pythogenic origin. Subsequently, she stated that a few days previous to her attack, she, passing through a neighbouring village, felt a sudden indescribable sensation of illness on stepping over a gutter or open sewer, and was never well after. That gutter trickled down one side of that village street from an open privy half-way up the village, and for years that side of the village, below the privy, had never been free from typhoid. Many apparently spontaneous cases may have a similar origin. Meanwhile, our course is clear: let us all act energetically on Dr Budd's disinfecting principles. Once arisen, typhoid fever is unquestionably supremely potent in spreading its own germs, and yet can be thus arrested. But let us not be contented with this alone, but also use our best endeavours to exclude the products of pythogenic fermentation from our houses, assured that only by so doing shall we be safe. We have much pleasure in recommending the study of this work to our professional friends, and to the public we could not give better advice than that contained in his concluding paragraphs. Speaking of the National thanksgiving, he says, "That the people of this kingdom should testify, in this most solemn of ways, their deep sense of the responsibilities they have in this matter is well. But if they wish to set a seal on the sincerity of their piety, there is—I say it with all humility—a still higher attitude to take. It is, to spare, from this hour, no human efforts to put under foot this great enemy of man. And let no one suppose that this is a matter in which he has no personal interest. The duty itself we may evade, but we can never be sure of evading the penalties of its neglect. This disease not seldom attacks the rich, but it thrives most among the poor, . . and he that was never yet connected with his poorer neighbour by deeds of charity or love, may one day find, when it is too late, that he is connected with him by a bond which may bring them both at once to a common grave."

An Introduction to the Study of Practical Histology for Beginners in Microscopy. By JAMES TYSON, M.D., Vice-Director of the Biological and Microscopical Section, Academy of Natural Sciences, Philadelphia, etc. Philadelphia: J. B. Lippincott and Co. London: Trübner and Co.: 1873. Pp. 50.

DR TYSON is already known in this country as the author of an admirable little book on the history and present state of the Cell Doctrine, in which he showed that he possessed a good general knowledge of histology. He now attempts, in the volume under review, to give a condensed account of the preliminary knowledge necessary for a study of the structure and functions of the various elementary tissues. He has evidently made free use of the larger text-books of histology, and of the writings of German histologists in particular. He has succeeded in writing a useful book. On the selection of a microscope, he gives good advice in recommending the instruments of Nachet or of Hartnack, the successor of Oberhäuser. The instrument of Zentmayer, figured in the frontispiece, appears to us, so far as one can judge from a drawing, to be serviceable, except that the stage is too high. The stage of a microscope should be exactly the height of the palm of the hand, so as to allow the edge of the hand to rest on the table, and thus avoid fatigue in moving the slide on the stage. The double nose-piece for the objectives, recommended by Dr Tyson, is not so useful, even for a beginner, as to warrant the additional expenditure. In giving directions as to the mode of estimating the magnifying power, the author is not sufficiently precise for a beginner. He does not state how the magnified divisions of the micrometer are to be measured off—namely, by a pair of compasses. He then passes on to the microscopical study of blood and epithelium. In describing the colourless blood-corpuscles, he adopts the description given of these bodies by Klein in Burdon-Sanderson's Handbook of Practical Physiology for the Physiological Laboratory. It has always seemed to us that this division of these colourless cells into four kinds is very artificial, and only serves to confuse the student. No doubt colourless cells are found, often even in human blood, of various sizes, some transparent and slightly granular, while others are highly granular, but all intermediate varieties are also met with. With regard to the colourless cells in human blood, Dr Tyson states that the granules in their interior "are subject to the well-known 'molecular motion,' or 'Brunonian' movement." This we believe to be an error. A colourless corpuscle consists of a mass of jelly-like protoplasm, of uniform consistence throughout, having embedded in it small molecules or granules at different depths. The appearance of molecular movement is due, not to movements of the molecules in the mass, but to rolling movements of the corpuscle, so that molecules in one plane are seen to move in the reverse direction to those in another plane. The apparent movement of these granules is not at all analogous to that seen in a salivary corpuscle. The action of salt solution, distilled water, watery vapour, dense saline solutions, syrup, dilute acetic acid, alkalies, boracic acid, tannin, and carbonic acid on the corpuscles is very well described. With reference to the action of boracic acid on the frog's corpuscle, Dr Tyson describes the terminology of Brücke—namely, that the concentration of the colouring matter in the centre of the corpuscle is the zooid, which formerly inhabited the surrounding substance, termed by him the œcoid (*οικος*, a house). The action of tannin, first observed by Dr Roberts of Manchester, is not stated with sufficient distinctness. Dr Tyson states that four cells are to be met with in connective tissue: (1), The wandering cell; (2), The spindle-shaped connective tissue corpuscle; (3), The round, finely-granulated connective tissue cell; and (4), The cylindrical cell. To a beginner this is only confusing. We have grave doubts as to these cells being specifically different forms. Is there, for instance, sufficient reason for separating the so-called spindle-shaped coarsely granular from the round finely granular cell? The difference in shape may be due to pressure, or to the cessation of life when the cell was arrested in amœboid movement; and as to the phrases finely and coarsely granular, these are only relative terms. With regard to pigment cells, Dr Tyson makes no reference to the movements of the granules in the pigment cells of the frog, first described by Mr Lister. Dr Tyson divides connective tissue into trabecular tissue and fibrillar tissue. The trabecular tissue does not gelatinize on boiling, whereas the fibrillar does. Trabecular tissue is found in the mucous tissue of the umbilical cord, in Peyer's glands, and in many other structures. It is the "neuroglia" of Virchow, which binds together the histological elements of the nerve-centres. Fibrillar connective tissue is the variety best known; found in the form of long wavy fibres or bundles. Dr Tyson here gives many excellent methods of studying connective tissue, by the action of baryta water, salt solution, and staining by carmine, chloride of gold, and hæmatoxylin. He then describes the German method of making sections of tissues by embedding them in a mass of five parts of paraffine and one of lard, enclosed in a little box made of stiff white paper. This little box is then held in the left hand, and a thin section is cut by a broad flat knife held in the right. It is astonishing with what dexterity and neatness sections may be thus made. If broad sections—as, for example, sections of an entire organ, such as a small kidney—are required, it is necessary to use a section cutter, of which there are many varieties, but none of which are more serviceable than that of Mr Stirling, of the Anatomical Museum of Edinburgh University. Excellent directions are given by Dr Tyson as to the preparation of muscular tissue. He does not allude to the fact that beginners often find it extrèmely difficult to identify involuntary muscular tissue, especially if it exist amongst connective tissue. In describing the alimentary canal briefly, Dr Tyson states that "in the case of the small intestine, the following layers can be demonstrated with perfect facility:—1, Epithelium; 2, membrana mucosa; 3, muscularis mucosæ, or internal muscular coat, composed of two layers—*a*, circular, *b*, longitudinal; 4, sub-mucous connective tissue; 5, muscularis externa, also composed of two subdivisions—*a*, circular, *b*, longitudinal; and finally, 6, the peritoneum." We can imagine but not describe the disappointment felt by a beginner, nay, even by one of great experience, in histology, after he has laboured for hours in trying to see, with his own eyes, all these coats; and yet he is told they can be "demonstrated with perfect facility!" Dr Tyson now proceeds to describe the various processes for staining the tissues—by chloride of gold, nitrate of silver, hæmatoxylin or logwood, and aniline. There can be no doubt that the method of staining by logwood, first invented by C. F. Müller, is very valuable, more especially in preparations of cornea and nerve-centres. While the description of fresh nerve—that is, nerve examined immediately after its removal from the body of an animal just killed—is very correctly given, we cannot agree with Dr Tyson in the statement that the axis cylinder of a nerve fibre or tube has a fibrillar structure. In this view, however, he agrees with many histologists, more especially those of the Vienna school. When a living nerve is examined, no trace of such a structure is apparent, even when scrutinized with the twelfth of an inch of Ross, and with the aid of various methods of illumination. No doubt, hardening in absolute alcohol, steeping in oil of turpentine, and immersion in dammar varnish, may produce occasionally a pseudo-fibrillar structure; but this appearance is due, we hold, entirely to the action of these reagents. Brief directions are given as to the methods of preparing bone, teeth, and cartilage. A little more might have been said with regard to cartilage. It is the first tissue which a beginner in the art of cutting sections should employ; and a study of the life history of cartilage teaches many fundamental principles in physiological histology. Finally, Dr Tyson describes very shortly the mode of preserving specimens, far too shortly to be of any practical use. In conclusion, we cordially recommend this little book to any one who desires to become acquainted with the great progress which has recently been made in histology, and who may not have the time or opportunity of consulting the larger treatises of Beale, Frey (recently translated), or that of Klein, in Professor Burdon-Sanderson's Handbook for the Physiological Laboratory. The great advance in histology recently made is due to improvements in the methods of preparing the tissues; but many of the appearances now figured by histologists probably do not represent structure as it is in the living body, any more than the compounds produced by the chemist in the examination of urea by the use of reagents in his laboratory represent substances existing in the blood. Many of these appearances and of these compounds are artificial, the result of the processes employed in the examination.

Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D. Sixth Edition. London: Longman, Green, and Co.: 1874.

THE finding on our table of a new edition of Dr West's work this year considerably added to our Christmas pleasures. Like the appearance of an old familiar friend, we at once anticipated much agreeable intercourse, and have spent several happy hours in perusing the well-known pages, and noting the changes which years have made. It is a book which bears re-perusal well. There is a charm in it, other than lies in the easy and graceful style, and the admirable portraiture. It is devoid of the mannerism of a treatise, the irksomeness and heaviness of a compilation, yet lacks little, does full justice to the opinions and labours of others, and is rich in references to works in many languages. It is from the fulness of the author's own experience that he speaks—an experience not more remarkable than the accuracy and minuteness of his observations.

This personal character of the work gives it great value, but it is enhanced by an additional peculiarity. It reflects so fully, not only the subject, but the author's mind and character, that you feel that you become familiar not only with the matter, but also with the man who is speaking to you. Those mental qualities which are so pleasantly portrayed, but of which this is not the time to speak, command confidence and respect, even when you feel most inclined to differ from the opinions expressed. But the more frequently and more carefully the book is perused, the less frequently we have found, in our own experience, do we feel inclined thus to differ. When Dr West's opinion is fully formed, he states it strongly; when the statement of a general principle is sufficient, he does not waste time in many details. Thus, in treatment he often enters minutely, and, in the first place, to that directed towards the severer and rarer forms of the affection, and that for the commoner kinds he dismisses by a few general and comprehensive remarks. It is on this account, we believe, that the opinion has arisen, that in treatment Dr West is not a safe guide, and he does render himself at times liable to misapprehension.

The present edition gives evidence in nearly every page of a careful revision. Several parts have been re-written and some additions made. Among the former are the chapters on Paracentesis, and the pathology and treatment of Chorea; and of the latter, the Spasmodic Coughs of Children, with Asthma and its connexion with Eczema. Numerous paragraphs have received some alteration, and scattered throughout the book will be found many new ones in which the diagnostic value of the thermometer in various diseases, and of the ophthalmoscope in affections of the brain, receive notice. Verbal changes are to be found, such as, tubercular meningitis for acute hydrocephalus, and typhoid for remittent fever; while the distinction between tuberculosis and scrofula is more fully attended to in the use of these terms. In nearly all the instances also where the statistics of his practice is given, the numbers have been considerably increased. But as it is in reference to the treatment of acute inflammatory affections that the greater exceptions have been taken, and wherein Dr West's changes of opinion have been candidly expressed in former editions, it is on this subject the greatest interest will be felt, and which will best repay a careful review.

In the second lecture (p. 17), the following new paragraph is given:—

"Changes in medical opinion, such as have taken place within the past twenty years, influence one's conduct by slow and almost imperceptible degrees; and I find that my practice now differs much from what it was a quarter of a century ago; that I deplete less than I did, that I have less faith in mercury, that I employ antimony more rarely, that I have more confidence in nature's powers, less reliance on my own resources. And yet I am unwilling to believe that all my former observations were erroneous, and that my old faith was entirely misplaced; but, unhappily, I have no longer the leisure to test the value of these changes as I could wish; while the peculiarities of consultation practice in a large city, though they may sharpen one's perception, and increase one's promptness of action, are anything but favourable to scientific investigation or to accurate reasoning."

With characteristic candour and brevity, Dr West here states the changes we are to expect; and most fully do we accord to him the belief that the causes of those changes have not *entirely* lain in "error or misplaced faith," and concur with him that, notwithstanding the changing constitution of diseases, there still do occur cases in which depletion is of service, where mercury is not hurtful but beneficial, and where antimony, rightly used, is of value. In justice to our author, we must acknowledge that, having started in our professional career thoroughly imbued with the views which could only admit of the "error," the result of experience has been to reveal that the "old faith was not entirely misplaced, and that *all* the observations were not erroneous." At the same time, it must be admitted that experience is required to quicken the "perception" before one can gain the "promptness of action" which, in our author's hands, may be productive of so much good.

The chapter on Acute Cerebral Congestion has been in a great part re-written. In the treatment, abstraction of blood now obtains but the second place. The importance of other means, and many that are often thought trivial, are first duly insisted on. But the benefit of leeching in certain cases is still, and we believe rightly, maintained. Bromide of potass and small doses of aconite are recommended, and good service has been obtained by the use of packing in the wet sheet—(we prefer a blanket, as more agreeable.)

The lecture on Pleurisy is one of the most admirable in the volume. It is a disease, the peculiarities of which in children, as relating to symptoms and physical signs, and consequently the difficulty of early diagnosis, are but imperfectly understood by the bulk of practitioners. The caution he gives against the culpable carelessness with which pleurisy and empyema are often overlooked in children is worthy of notice; and not less so the statement he makes, that "there are no cases in which it happens to me to have to screen the mistakes of others so often as in cases of pleurisy and empyema."

All unprejudiced observers will justify him in upholding the value of bleeding (leeching) under certain conditions in this affection: "In all cases in which the child's previous health has been good, in which pleurisy is idiopathic, the symptoms are at all urgent, and the patient is seen before the occurrence of effusion." But, be it remembered that all these conditions must be present, and that the absence of any one of them would contraindicate all active proceedings.

The importance of correct and early diagnosis is here manifest, —a matter of no small difficulty in children; but the subject of diagnosis is so fully and ably treated, that it will well repay a careful study. To it, it would be difficult to add anything, and the lecture will be as instructive thirty years hence as at the time it was written.

The lecture on Pneumonia is less satisfactory. Viewed in the light of the more recent opinions, in the advance of pathology, and the careful clinical study and thermometric observation of the disease, it shows somewhat the lines of age. But the changes which are taking place with regard to the subject are perhaps not yet sufficiently matured to warrant the author in altering what he has already written.

In the treatment, he gives full effect to the changes in medical opinion:—

> "It is, I believe, but rarely at the present day that depletion is indicated in bronchitis or pneumonia; and tartar emetic needs to be given more sparingly than in former years, and acts with less certainty in cutting short at its very outset the inflammatory action." (P. 333.)
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> "But I confess that I cannot forget the good results which I saw years ago, from the abstraction of blood at the outset of an attack of pneumonia in previously healthy children. . . . I do not, however, think that after the first twenty-four or thirty-six hours this result will be obtained, and should not advise depletion at a time when small crepitation has become generally diffused, still less when dulness or bronchial breathing is perceptible; nor should I advocate a repetition of bleeding in any case where the good which it appeared to have effected had passed away." (P. 356.)

There is a nearer approach to truth in the position here taken than in entirely renouncing the use of blood-extraction. Rare as the cases may be, yet undoubtedly they do occur, in which much relief and benefit may be obtained by this means. We may bleed *in* pneumonia—even Professor Bennett has been known to do this—but not *for* pneumonia; against concurrent circumstances—not against, or with the view of arresting the disease.

Dr West has left his recommendation of antimony, in the early stage of this affection, unchanged. "Given in a dose of ⅛th of a grain every ten minutes till vomiting is produced, in the case of a child two years old, and continued afterwards every two hours for a period of twenty-four or thirty-six hours, it subdues the fever and abates the dyspnœa in a most remarkable manner: the minuter crepitation becomes larger, and, as M. Trousseau says, 'there is no stage of convalescence—the child dangerously ill yesterday is all but well to-day.'"

As regards this practice, in cases of great oppression of the breathing with high fever, with sudden onset, our experience is at one with our author of the benefit derived from antimony so used; but, with all deference to so high authorities, we must differ from the interpretation of the disease, and believe they are catarrhal in nature, certainly not croupous pneumonia; and we would place the recommendation under the treatment of bronchitis. One addition also we would make, that the dose be repeated not every two hours, but at the end of two hours *after the last vomiting*.

Whilst, starting, as we before said, from opposite ends, we have

so far reached a common stand-point with our author, and given concurrence to his views, we must take entire exception to his continued belief in the use of mercurials in acute inflammatory diseases not syphilitic. "I still believe," he says, "that it possesses a peculiar and specific power in controlling acute inflammation of the serous membranes of the chest and abdomen, and that both acute pleurisy and acute peritonitis yield to a combination of calomel and opium more speedily than to opium alone." In the dysentery of children, in laryngeal inflammation, or true croup, after the first active symptoms have been subdued, and in acute pericarditis, he also recommends it. There is, however, a contradictory statement as regards its use in pneumonia. At page 18 he says—"I do not regard it as of any service in acute affections of the pulmonary tissue, nor in any form of bronchitis." And at page 357—"In cases where the symptoms (of pneumonia) do not set in with such violence as to indicate the necessity for very large doses of antimony, or in which the disease has passed that stage where antimony so given is likely to be beneficial, *mercurials* may be used with great advantage." We take this discrepancy as an indication of an approaching complete change of opinion which will bring us again at one with our author.

There is truth, however, in the remark, and with him we shall "hold to the belief that in some forms of chronic non-tuberculous consolidation of the lung, recovery is expedited by the careful employment of mercurials;" but it is not to subdue the inflammatory action, but to remove its products, that we would give mercury. Nor would we limit it to this case alone; but wherever resolution in inflammatory products is tardy or arrested, no safer and more powerful remedy can be employed to quicken the action. Given in the fortieth part of a grain of corrosive sublimate three times a day for a fortnight only, it is an admirable tonic in the sluggishness of *scrofulous* affections.

In reference to the treatment of croup, Dr West has intentionally left his remarks unchanged. His views have been modified regarding it, "just as the treatment of bronchitis and pneumonia have been modified, by the changes in the epidemic constitution of disease which recent years have brought with them. . . . But with every allowance made for these changes, I still believe that a decided antiphlogistic treatment (by which I mean the employment of antimony in emetic doses, the subsequent administration of calomel, and, if the case be seen at the very onset, the recourse to actual depletion) is indicated in almost all cases of acute idiopathic croup."

We commend the author's views to the consideration of those who are inclined to regard croup as always essentially diphtheritic, believing he is right when he "insists on the difference in character between cynanche trachealis and diphtheria, and, as a consequence, on the necessary difference between their treatment," while, at the same time, the treatment of simple inflammatory croup must remain as a *quæstio vexata.*

Whilst the opponents of active antiphlogistic treatment will rejoice over the modifications in Dr West's views, it is incumbent upon them, at the same time, to note and consider the no less decided retention of a place for the most active measures. Nor is this due to any bias from the older opinions. It is easier to withhold than to put forth the hand; and not the least value of Dr West's last edition is the assistance it may afford in the study of what are the indications which warrant active treatment—a study which can advance only with experience, and will be duly appreciated by all who feel that there can be as much culpability in too much expectancy as in too great interference.

The Anatomy of the Lymphatic System. By E. Klein, M.D. London: Smith, Elder, and Co.

No part of histology has made more rapid advances of late years than that of the lymphatic system, and this notwithstanding the extreme difficulty of the investigation. Among the numerous writers on the subject whose names may be mentioned are those of Recklinghausen, Auerbach, Ludwig, and Schweigger-Seidel, and much variety of opinion exists among them as to the arrangement of the lymphatic vessels in different tissues, their mode of origin, and the relation in which they stand to the great serous cavities of the body.

The work before us contains the results of an independent research into many of these points, conducted by Dr Klein, in connection with the pathological inquiries of Dr Burdon-Sanderson, and constitutes one of the most important contributions to histological anatomy that has appeared for some time. The subject is treated of in four sections, the first of which is devoted to the consideration of the epithelium of the free surface of the serous membranes. This, says Dr Klein, does not consist everywhere of a layer of flattened cell plates, but in many places of club-shaped, or even columnar cells, provided with a clear, shining nucleus. These cells, constituting what the author calls "germinating epithelium," are particularly well seen on the trabecular portions of the omentum of the cat, dog, and guinea-pig. In these we find numbers of cells raised from the surface by means of a stalk, and possessing, in their peripheral portions, either two nuclei or a single nucleus in a state of division, along with cells of a spherical form, resembling lymphoid corpuscles, which are either attached to the general surface of the membrane, or are in process of separation from the young endothelial cells. On some occasions protoplasmic masses, with from ve to ten nuclei, and attached to a stalk or peduncle, are to be seen. Farther, there are always to be seen numbers of spherical lymphoid corpuscles, which are detached from the surface, having been perfectly separated, showing that germination has been proceeding with great rapidity. A similar arrangement of these elements is also to be seen in the pleura of the cat, dog, and monkey, as also in the mesogastrium of the frog; but here the epithelium is for the most part ciliated, and oftentimes polyhedral.

In the mesentery of the frog, when stained with silver, there is often to be seen, at the point of junction of the endothelial cells, a branched corpuscle, the body of which is situated at this point, whilst its processes stretch between the cells, and become identified with the lines indicating the separation of the latter. These corpuscles vary in shape, as well as in the number and form of their processes and nuclei. Sometimes they are situated deeper than the endothelium, whereas one or other of the processes extend superficially between the cells of the latter. A similar arrangement of these branched corpuscles is seen in the mesentery of the cat, as also with great frequency in the omentum of the rabbit, and the pleura mediastini of the dog, cat, and monkey.

In the second chapter the author discusses the nature and structure of the cellular elements of the ground-substance. When the omentum of the rabbit is carefully examined, a number of opaque patches are seen, which at various points coalesce into tracts. These latter, for the most part, follow the course of the larger bloodvessels. The patches exhibit numerous brownish structures of an irregular shape, which contain larger and smaller brownish granules, and in most of them a nucleus, sometimes provided with a nucleolus, is to be seen. The cells are irregular in shape, and their body is beset with a number of larger or smaller blunt prominences. They correspond in shape to migratory cells. In addition to these, there are to be seen a small number of granular corpuscles, with one or two nuclei, which closely resemble lymphoid corpuscles. They are often so closely connected with the large cells that they resemble knobs, which are in a state of separation from them. In the ground-substance, which is composed of a network of branched, more or less flattened, cells, is to be seen a rich system of clear lacunæ, which communicate freely with one another. In these there are to be seen finely granular branched cells, along with small lymphoid corpuscles; spherical cells, varying in size from that of a small nucleus, surrounded by a thin zone of protoplasm, to twice the size of a colourless blood-corpuscle, are also to be seen in the lymph-canalicular system, and seem to be derived from the branched cells of the matrix. To these tracts and patches just described the author gives the name of *lymphangial* tracts and patches, and distinguishes them as of two kinds, for a description of which we must refer to the work itself. In these tracts, as observed in the omentum of the rabbit, migratory cells are seen to be connected to the branched cells of the matrix, and are therefore probably derived from them. In the pleura of the cat the lymphangial nodules consist of a delicate reticulum of nucleated branch-cells, the meshes of which are filled with lymphoid corpuscles. These are sometimes seen to be connected with the branched cells, and may therefore be regarded as derivatives from them.

As regards the formation of fat in the omentum, it is produced by the transformation of the branched cells of the matrix into fat cells. The development of the lymphangial tracts into fat tracts varies in different membranes, the tendency in this direction being greater in the peritoneum than in the pleura. So long as the tract does not become converted into fat lymph, corpuscles are to be seen. These disappear, however, so soon as this transformation takes place. The lymphoid cells are not, however, converted into fat cells, these last being, as already stated, transformed branched cells.

In the third chapter the lymphatic vessels are considered in their relation to the tissue of their membrane, as also to the bloodvessels. These may either run alongside of the lymphatics, or even occasionally *within* the lymphatics. As regards the relation of the lymphatic vessels to the surface of the serous membranes, Dr Klein distinguishes on the surface two kinds of stomata—stomata vera and stomata spuria, the former of which represent the mouths of true lymphatic vessels, or of lymphatic lacunæ.

The bloodvessels of the serous membranes are described in the fourth section; whilst the fifth is devoted to a description of the changes undergone by the lymphatic elements in pathological conditions.

The work is illustrated by ten beautifully-executed plates, and forms a most exhaustive treatise on the subject under consideration.

On the Relation of Plants and Animals to Inorganic Matter, and on the Interaction of the Vital and Physical Forces: Being an Introductory Lecture to a Course of Physiology. By J. BELL PETTIGREW, M.D., F.R.S., F.R.S.E., F.R.C.P.E., etc. Edinburgh: Maclachlan and Stewart.

DR PETTIGREW has chosen for his opening lecture a theme at once embarrassing and difficult. It is seldom that lecturers venture to address a youthful audience on such abstruse subjects. But it is due to Dr Pettigrew to say that he has acquitted himself with tact, discretion, and judgment. What artists in their studios term "breadth of treatment" is the characteristic of his lecture. It is the philosophic expression of a much-vexed question. Add to this, Dr Pettigrew's literary style, with its quaintness of phrase, fertility of illustration, and logical symmetry, and the production may be compared to that of the "old masters." It argues well for his success as a teacher.

En outrance, Dr Pettigrew maintains that the time has come when the study of physiology must revert to its ancient comprehensive meaning—"science of universal nature;" no longer must it be wrapt in the swaddling-clothes of the eighteenth century. The practical inference, then, well substantiated by the labours of our Continental brethren, is, that our students necessarily need better drilling in mathematics, physics, chemistry, botany, zoology, and, shall we say, Mill's logic—preparatory to applying themselves to the cosmic and fertile field of physiology.

Dr Pettigrew's views of the relations subsisting between the organic and inorganic kingdoms, briefly worded, are—correlation and interaction. As a corollary, he considers that the organic cannot be produced from the inorganic by purely physical forces. According to the recent positions of physiological sects, it seems to us that the opinions announced in his lecture are more thoroughly in harmony with the actual state of our biological knowledge than those maintained by the so-called physicists or vitalists. Our lecturer's physiological *credo* is virtually a golden mean between these two opposing schools. The views of the vitalist are suggestive of the past; those of the physicist loom as a possible future. According to our reading, Dr Pettigrew has managed to give the most accurate and best-formulated expression of what the present can safely say regarding those very abstruse problems, life and nature.

By a series of stepping-stones he traces the ancient doctrines on life to modern times, and compares the ideas promulgated by Newton and Swedenborg to those of Plato and Aristotle—by Haeckel and Tyndal to those of Democritus and Epicurus. The *tour de force*, however, of the lecture centres in the moot point, "protoplasm," and this is treated in a terse, telling manner. Among the band of debaters on the debatable substance—material essence of life—Huxley and Beale having played prominent parts, their opinions receive a due share of the author's attention. To Professor Huxley's enunciation respecting "protoplasm," this primary life-basis, that it is single, yet to all intents and purposes multiple, Dr Pettigrew dissents. The latter lightly calls attention to the fact that, from this supposed basement-material, with its threefold unity—faculty, form, and substance—there originate the most dissimilar myriads of organic forms. He opines that, even granting possession of threefold unity, according to our present methods of research, this but implies differentiation. If protoplasm were absolutely identical in ultimate composition, it is difficult to conceive and believe that there could be produced from it, by the operations of the primal life-force, the countless differentiated results the biologist has to deal with. In short, Pettigrew's interpretation of Huxley's dicta is to the effect that they contradict themselves. Dr Pettigrew's statement of Dr Beale's views on his primal substance, "bioplasm," seems to us remarkably fair and moderate. The only objection that can be urged against the statement is, that it puts them much more clearly than Dr Beale himself does, and makes their obvious absurdity stand out all the more manifestly in consequence. The phenomena, the sum of which we call life, do most certainly, so far as we know at present, involve the existence of non-living matter, for they are never manifested apart from a physical basis or substratum, four-fifths of which Beale himself asserts is non-living. A glance at Dr Pettigrew's teaching shows how much sounder and freer from ambiguity and vagueness it is compared with that of Dr Beale's.—(*Vide* p. 17 of the Inaugural Address.)

An extremely able and fair summary of the spontaneous-generation controversy is given; and though we infer that Dr Pettigrew is inclined to be a "Panspermist," the upholders of the opposite doctrine have no reason to complain of his unfairness in treatment, or the ability with which he states their case. The dispute in question is one that is not likely ever to be satisfactorily settled. The whole resolves itself into one of credibility of equally credible but conflicting witnesses; therefore it is very difficult for the critic to come to any positive conclusion. We suspect the importance of this controversy is vastly over-estimated. When we read of those experiments that the leaders of the two parties are constantly making with such dauntless zeal and ingenuity, we are apt to cry, *Cui bono?* Most biologists are nowadays evolutionists or non-evolutionists. The latter are always at liberty to say, the sources of fallacy are so numerous in the experiments, and the difficulty of overcoming them so insuperable, that they would deny the accuracy of the results, by whomsoever asserted or howsoever attested. As for the evolutionists, their interests in the proof of spontaneous generation cannot be great. They must, if they are thoroughgoing and logical, admit that, in the far-off past, there must have been a set of conditions existing in which spontaneous generation, or the evolution of organic from inorganic matter, took place. Hence it follows whether a chemist or physiologist can now reproduce those conditions, near enough to be confidently averred or vouched for, is a matter of comparatively little importance in the abstract. Unquestionably, under the present phase of facts, the balance of the evidence is in favour of Dr Pettigrew's statement, that "living organisms cannot be produced from purely inorganic matter by purely physical forces." Still, as Bacon says, "A wise seeking is the half of knowing," therefore let the heterogenists work on. Their researches may lead to wonderful results, of such a practical nature as may astonish themselves quite as much as their opponents.

Regarding the great question, What is life? we are still in doubt, for Dr Pettigrew, like his predecessors, only goes so far towards its solution. In fact, this is one of those questions which shows that the fruit of science is not yet sufficiently ripe to be shaken off the tree of knowledge; if, indeed, this golden apple is ever meant to gratify and satisfy the inquiring appetite of man. Notwithstanding our lecturer's store of biological knowledge, and his subtle faculty of reasoning, we are doubtful whether a more rational or philosophical formula is given than is found in the old once-celebrated work of Coleridge—his "Essay towards the Formation of a more Comprehensive Theory of Life," which, vague and hazy as it occasionally is, yet merits the attention of our more highly trained scientific physiologists. No one can accuse this great poet and conversationalist of materialism. Nevertheless, thus he writes: "Life is merely another form or process of such physical forces as heat, electricity, chemical affinity, etc. The only difference (according to Coleridge, being) that these forces *act on and through bodies, which are made into independent units, separated from, though partaking in, the Being of the material universe*, by what he calls a certain 'principle of individuation.' Hence, the higher the animal the more perfectly is it individualized *and separated from the material world*." He says to a reflecting mind, "The very fact that the powers peculiar to life in living animals include coherence, elasticity, etc., would demonstrate that, in the truth of things, they are of the same kind, and that both classes (organic and inorganic) are but degrees and different dignities of one and the same tendency. For the latter are not subject to the former as a lever or walking-stick to the muscles; *the more intense the life is, the less does elasticity, for instance, appear as elasticity;* it sinks down into the nearest approach to its physical form, by a series of degrees, from the contraction and elongation of the veritable muscle to the physical hardness of the insensible nail." . . . "Unless, therefore, a thing can exhibit properties which do not belong to it, the very admission that living matter exhibits physical properties includes the further admission that those physical or dead properties are themselves vital in essence, *really distinct*, but in appearance only the same, or in absolute contrast with each other."

Inadvertently or intentionally we know not, but, at all events, the name of Oken does not figure in the lecture. Oken, however, is one of the boldest speculators of the theory of the universe and life. We call attention to his "Phisio-philosophy," not certainly in the support of proved truths, but rather to show how the borrowed light of this great visionary seems to have illumined some of our insular materialistic stars who aspire to a first magnitude. So the world of science goes round. The great benefit of such a lecture as Dr Pettigrew's consists in its healthful, cheering tone; physiology is clothed in the garb of the great mother of nature, not wrapt in inscrutable nothingness. By a few masterly touches, student and practitioner alike are put in possession of realities, and the duty enforced to hold by logical data in the physiological application of medicine to the ordinary affairs of life.

Memorials of the Life of James Syme, Professor of Clinical Surgery in the University of Edinburgh, etc. By ROBERT PATERSON, M.D., Vice-President of the Royal College of Physicians, President of the Harveian Society, etc. Edinburgh: Edmonston and Douglas: 1874.

RECEIVED as it has been so late in the month, just before we are going to press, we can do little more than notice the publication of this work, on a subject so intensely interesting to most of the professional world of Edinburgh. The Harveian oration of 1873 has formed the nucleus of this volume, and there are some faults in its arrangement and literary management which are explicable by this fact: it has become a volume rather by a process of accretion, or addition, than by legitimate growth. Thus, while in most respects it gives an accurate enough account of Mr Syme's life, the information is given in such a desultory, unconnected manner, that it is a little difficult to follow.

Those who know the genial author will know with what a loving, reverential touch his old master's life has been handled, and we could hardly wish it otherwise; and those who know how this work has been written in the *horæ subsecivæ* of a busy, well-filled life, will appreciate the labour it has cost and the devotion it implies. Still, in some points we could show errors, both of judgment and of fact. The controversy with Mr Miller is told in such a way as to leave the unpleasant impression that Mr Miller had been disingenuous to begin with, and had confessed as much in his final interview with Mr Syme.

Again, surely it was not by the death of Professor Turner, in 1842, that the chair became vacant which Mr Miller gained, as Sir Charles Bell had occupied it from 1836 to 1842.

There is too much of controversy—too few, unfortunately, of his short, characteristic letters, that Mr Syme could write so well. One exquisite letter from Carlyle to Syme we must transcribe: it came when the clouds began to gather at the close of a singularly happy and prosperous life, when Mrs Syme died, only three months before his first paralytic attack:—

"CHELSEA, *5th February* 1869.

"Dear Mr Syme,—My heart is sore for you, ever since that lamentable news arrived. Oh, what a loss, what a loss!—the brightness of your life, as it were, suddenly extinguished—changed into mournful memories—unspeakably mournful—though by degrees (if you could believe me) beautiful too, and even blessed! A loss to you meanwhile which is immeasurable and irremediable. Alas! alas! I myself, little did I think it was the last I was to see of that bright, affectionate, beneficent, and noble spirit, in this world.

"I judge well you will rally yourself, and bear in silence, like a brave man, the inexorable and inevitable; but it is sore to flesh and blood. May God bless you and keep you!

"Words of condolence, I know sufficiently, are idle, often almost worse, and I will add no more of them. I wished you to understand that perhaps no one of your many friends more deeply sympathizes in this saddest of events.—Yours ever truly, T. CARLYLE."

Would we had in the Memorials less of the controversies and heartburnings, and more about the well-chosen, tried, and trusted friends. Few men could be more cautious in selecting, and still fewer so faithful in grappling, a friend.

Desultory, fragmentary, sometimes even unconnected, as these Memorials are, they will be read by the profession with the deepest interest, and many will thank Dr Paterson for his labour of love.

The work is illustrated by a good, calm, sagacious photograph by Adamson, engraved by James Faed, and a copy of the somewhat idealized and effeminate engraving by Holl of Richmond's drawing of Mr Syme in his prime.

One or two typographical errors will need to be corrected before a second edition appears.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING IV.

Wednesday, 4th February.—Dr RUTHERFORD HALDANE, *President, in the Chair.*

I. *Dr James Ritchie* showed a very good example of ENLARGEMENT of the so-called MIDDLE LOBE of the PROSTATE, in which the enlarged portion acted like the ball in a ball-valve. The preparation was taken from a patient æt. 68, who had suffered for two years from gradually increasing difficulty of micturition, and latterly from complete retention. The prostatic portion of the urethra was not distorted; and, as the enlarged portion in the recent state was movable, it did not offer an obstruction to the introduction of a catheter. Secondary hypertrophy was marked. When the patient came under Dr R.'s care, the urine contained multitudes of bacteria, and was alkaline, consequently the mucous membrane showed a considerable amount of irritation.

From the same patient there was also shown a SMALL CALCULUS, taken from the right ureter just at its origin from the pelvis of the kidney. During the last four years of his life, this patient, on performing any sudden movement, had been subject to an attack of pain across the upper part of the abdomen, but chiefly at the right side. Only on two occasions was the pain referred to the direction of the ureter or of the cord, and on the last of these occasions, viz., about three weeks before death, constipation, which had lasted several days, was succeeded by complete obstruction. There was then pain in the right iliac fossa and right flank. Three days later the patient had a rigor of ten minutes' duration; and, at intervals of three days, a second and third rigor. There was gradually increasing dulness in the painful region, viz., over the cæcum and ascending colon, but no distinct tumour could be felt. The obstruction was diagnosed to be due to local peritonitis, and was treated with opiates and fomentations; and, at the end of ten days, a warm-water enema was followed by a copious partly-formed evacuation. Hiccup and vomiting, which had occasionally occurred during the previous ten days, now became frequent and severe. The patient gradually sank, and, three days before death, right hemiplegia began slowly to supervene.

On examination, there was a small quantity of lymph around the ascending colon and at the hepatic flexure; and on turning the colon aside, there was found an abscess containing about 14 oz. of pus. The abscess was situated in front of the lower end of the kidney, and extended downwards a short distance. It was lined with a thick layer of lymph; and in the posterior wall was the ureter, with the calculus at its upper part.

II. *Dr Muirhead* then read a paper by Dr Lawrence of Montrose, entitled, REPORT OF A CASE OF INTRACRANIAL TUMOUR, with remarks, which appears at page 809.

The *President* observed on the importance and interest of the case, and on the difficulty of distinguishing the symptoms, from the length of time during which they gradually made their appearance. When listening to the case, his own opinion was that the lesion rather involved the central ganglia than the base, as there were few symptoms which are supposed to be characteristic of disease of the base of brain. The case was rather incomplete, especially as regarded post-mortem appearances. The fluid in the ventricles was a dropsical symptom, the result of pressure on the veins, and its slow progress permitted the brain gradually to accommodate itself so as not to affect the mental functions. The fatal result was probably due to exhaustion, and not to the small apoplectic clot. He alluded to a case somewhat similar, which he had seen nearly twenty-five years ago, then considered a tubercular one; but, from what is now known, it was probably a syphilitic one. Dr Lawrence's case was probably not one of meningitis, but originally a tumour.

Dr Matthews Duncan remarked on the extraordinary variation in this and other similar cases, as regards the symptom of pain. Dr Duncan took exception to a statement in the beginning of the paper, "that every case of this kind has a different history." Were this the case, diagnosis would be at an end, and physical science could make no progress. If we knew the history of such cases and could trace their symptoms properly, the cases would not appear so different and various. The laws guiding these cases are as certain as in others, only our powers of observation are still too feeble.

Dr Batty Tuke.—This is not a satisfactory case as viewed by the light of Ferrier's experiments, as none of the symptoms can be ascribed to irritation of what he calls psycho-motor centres. It may be lugging the subject in by head and shoulders, but I am desirious of making a statement to this Society as I see the accuracy of Ferrier's observations called in question by Dr Dupuy, a student of Brown-Sequard's. He asserts that he has failed in obtaining the same results as Ferrier: if he has done so, it is from some shortcoming in his method or procedure; for, along with other members of this Society, I had last autumn the pleasure of seeing Ferrier perform all the experiments which he recounts in his paper. Dupuy says he has not been able to procure opening of the mouth and protrusion of the tongue by the stimulus of faradization—this Ferrier caused a dozen times with the utmost certainty. Whatever opinion we may hold as to the conclusions which have been drawn from these experiments, there cannot be a doubt of the absolute accuracy of the facts as narrated by Ferrier.

Dr G. W. Balfour deprecated the idea of appealing to experiments on the brains of animals for an explanation of the phenomena observed in cerebral disease in man. It was very well known that the experiments of Schiff and Magendie on rabbits showed that in these animals the corpora striata might be removed and yet sensation remain unimpaired, and the power of motion complete; and the experiments on the corpora striata of pigeons recently shown the Society by Dr M'Kendrick went far to prove that in them such injuries affected the sight; yet in man disease of the corpora striata left the sight unaffected, but was invariably accompanied by paralysis of the muscles on the opposite side of the body to a greater or less extent corresponding with the situation and amount of the lesion. It was a curious fact, that the sella turcica and the region about the cavernous sinuses was that specially affected by syphilitic deposits. So well was this known that at least seven out of ten of all affections of the ocular nerves passing through these regions were ascribed to a syphilitic origin; and he himself was accustomed in this matter to rely much more upon the physiological phenomena than upon the statement of the patient. He regretted that the precise form of strabismus present had not been indicated, and also that the condition of the retina had not been duly investigated during life by the ophthalmoscope.

III. *Dr Cappie* then read a most exhaustive paper on the RELATION of the CRANIAL CONTENTS to the PRESSURE of the

ATMOSPHERE. In it he detailed at some length the views and experiments of Dr Kellie of Leith, proving that the weight of the atmosphere serves to maintain the plenitude of the vascular system within the head. The balance of pressure within the skull is preserved by the absolute fulness of its cavity (owing to which circumstance exactly as much blood must make its exit from the sinuses as is made to enter by the arteries): also, by the activity of certain dynamic forces within the skull itself. After showing that the intensity of the intracranial circulation is determined by the greater or less activity of the molecular changes in the brain-tissue, and that the amount of blood circulating in the capillaries of the latter may be modified by variations in the amount of blood in the veins of the pia mater, Dr Cappie proceeded to discuss what effects are likely to be produced by the pressure of the atmosphere, as directed, on the one hand, through the arterial, and, on the other, through the venous vessels. Dr Cappie concluded a most interesting paper, which will appear in a subsequent number of this Journal.

The *President*, after thanking the author, expressed the difficulty there was in discussing a paper like this offhand, without re-perusal.

Dr Batty Tuke asked how far the author's views were modified by the discoveries of the lymphatic system, lymph spaces, etc., so recently demonstrated in the brain to such an unexpected extent.

Dr Cappie replied that these discoveries have not in the least modified or altered his views.

Dr Matthews Duncan referred to experiments which have demonstrated the variations in the intracranial fluid of children, and to those of Professor Leyden, lately published in Virchow's Archiv, in which he has attempted to measure the amount of constant pressure in the healthy human cranium.

IV. *Dr Joseph Bell* read a paper on the use of the ASPIRATOR in cases of URGENT RETENTION OF URINE. This paper will appear in a subsequent number of this Journal.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING V.

Wednesday, 14th January.—Dr J. MATTHEWS DUNCAN, *President*, in the Chair.

I. *Dr James Young* showed a MOLE, which, before removal, had occasioned a good deal of hæmorrhage.

II. *Dr J. Matthews Duncan* exhibited and explained HECKER'S NEW SLING FOR DIFFICULT BREECH CASES, with improvements by Poppel and Gregory. Dr Duncan said that a great variety of means had been suggested and devised for such cases—that was, where the hand was found to be quite inadequate. He himself had succeeded with the hand, but others had had occasion to resort to instrumental means, such as the blunt hook or the forceps. Hecker preferred a different plan, and had re-introduced (for his apparatus was a re-introduction) the sling. This was made of silk, and it was pushed up and over the anterior thigh by an instrument resembling Belloc's one for plugging the nostrils. The beauty and ingenuity of the instrument lay in the fact that it readily locked and unlocked, and might be used also as a blunt hook. Gregory had added the bit of bone at the extremity of the sling, which rendered it more easy of passage than when the silk was rolled up simply. Dr Duncan warmly commended the instrument.

Dr Cairns could not see any advantage in the sling over the hand. Those tapes were deficient in elasticity, a quality of the highest importance. The article was not new; and, in fact, something better had been introduced to the notice of the Society about four years ago. This was a piece of clock-spring covered with gutta-percha, and, when introduced, it turned round naturally, and enabled one to catch the end. He believed, however, that the hand was usually sufficient, especially if it were a small one; and he referred to a very difficult case which occurred in his own practice lately, where Dr Milne succeeded thoroughly with the hand alone.

Dr Thomson thought that Hecker's instrument was an ingenious one, and might prove of considerable service in the more difficult cases—viz., those where the hand failed.

Dr Charles Bell could perceive no superiority in Hecker's instrument over the blunt hook. There was a risk, he thought, that, when endeavouring to pass it over the thigh, it might go too far, and impinge on the other leg. It ought to be so adapted in its curve as to embrace either one thigh or both. He was of opinion, also, that there would be some difficulty in getting the ligature introduced. Dr Cairns had eulogized a small hand, but he begged to cite the opinion of an eminent London obstetrician—the late Dr Ferguson—that, in midwifery, a big hand was frequently of more utility, because it possessed more power.

Dr M'Rae had hitherto managed all his breech cases without any kind of apparatus; but he could conceive of circumstances rendering such an instrument as that of Hecker's of no little value.

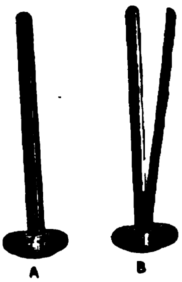
Dr Jamieson had recently met with two very difficult breech cases, where, he had no doubt, the instrument shown would have helped him greatly. In one of these he applied the forceps; in the other he introduced a handkerchief over the leg, and thus succeeded in effecting delivery.

Dr Milne had dealt with many breech cases in his own practice and that of others, but had always succeeded with the hand.

Dr Duncan replied that there had been a misunderstanding in reference to his statement of the function of Hecker's instrument. As he said himself, he had always succeeded with the hand; but rare cases would cast up, where, owing to great obstruction at the brim, the hand would be found quite inadequate; and hence the value of such an instrument as that which he had exhibited to the Society.

III. NEW FORM OF GALVANIC PESSARY. BY DR JAMES YOUNG.

Some years ago I introduced to the notice of this Society a new form of intra-uterine pessary, the only difference being the formation of the bulb.[1] Sir James Simpson on that occasion allowed that he had had frequently great difficulty in introducing the usual intra-uterine pessary, in consequence of the size of the bulbous head. Made as I ventured to recommend, the difficulty is entirely removed. A serious disadvantage, however, has always been present to my mind, namely, the retention of the intra-uterine pessary *in situ* only with the aid of a shelf gutta-percha support. To all who have been in the habit of using this valuable instrument, the difficulty referred to must have proved annoying. I now introduce to your notice a form of pessary, A, made by Gardner, cutler, South Bridge, in which, you will observe, the stalk is divided into two halves; one blade is soldered hard on the bulb, while the other half (or second blade) is worked on an internal plate, which, by the rotation of the milled button at the end of the stem handle, is easily opened up, presenting somewhat the appearance of the letter V. In place of *one* aperture in the bulb of the pessary, there are *three* in this instrument. The central square aperture is for the central rotating pin of the handle, and the two side openings on the outer bulb are for grasping the whole pessary, which is introduced through the cervix uteri, and by the rotation of the button the pessary is opened up, as shown in the woodcut, B. From the formation of this pessary when open, it will be seen that it naturally retains its position, the two blades being apart; and when it is to be removed, the slightest traction by the fingers will withdraw it, or, if necessary, you can introduce the stem, turn the screw, and so close the blades; but that will seldom be required, especially as the blades possess a degree of adduction as well as resiliency. The instrument for the introduction of the galvanic and stalk pessaries, consists of a steel tube with a central rod, square at one end to fit the central aperture of the pessary, and two wing pins to grasp the outer bulb, which retains the pessary in position, and allows the rotation of the inner plate, so opening the stem of the pessary. The rotation is effected by a milled button at the outer end of the handle, the instrument being held firm by a large scissor ring for the forefinger of the left hand.



Dr Jamieson inquired if Dr Young was sure, that such an instrument as he had exhibited had not been previously described in a medical journal as the invention of another. Dr Young replied, that so far as he was aware, his instrument was quite an original invention.

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[1] Edinburgh Obstetrical Transactions, vol. ii. p. 47.

Dr Dickson thought the instrument ingenious, but questioned if there would be any galvanic action on the uterus. He suggested the addition of a bit of ivory betwixt the two metals, in order to ensure this desirable end.

Dr Charles Bell considered that the Society were much indebted to Dr Young for bringing this ingenious instrument before it. He doubted, however, if it could be easily introduced in the very young female, and he suspected that, by the excitement it would induce, an increase of secretion from the cervix and a dilatation of the os would occur, rendering it difficult to retain it in the uterus. In his opinion, the question before the Society was, the consideration of a new instrument, and not the propriety of using it, which would entirely depend on circumstances.

During this discussion, several Fellows expressed themselves strongly against interference with the virgin uterus, unless under the pressure of imperious necessity.

IV. *Dr Duncan* exhibited and described TWO UTERINE MALFORMATIONS which were somewhat rare—the uterus unicornis and bicornis.

Dr Charles Bell asked if Dr Duncan had any history of the first case, and Dr Duncan said he had not.

Dr Cairns said that Dr Duncan deserved the thanks of the Society for his extremely interesting contribution.

V. NEW METHOD OF TYING THE UMBILICAL CORD WITH A VIEW TO PREVENT POSSIBLE HÆMORRHAGE, WITH PREPARATIONS.—*Dr Dickson* said, Having had three or four cases of bleeding from the umbilical cord, notwithstanding the application of the usual two ligatures, and as the medical attendant may be blamed quite undeservedly in such cases, I some time ago devised a plan which promises to prevent any such annoyance in future. A thick gelatinous cord is that which is most liable to bleed. Although tied very tightly by the "clove hitch" or otherwise, it shrinks away from the ligature; and the moment its diameter becomes less than that of the ligature, the vessels become pervious, and bleeding may ensue—I say *may*, because in the great majority of such cases it does not. With a view to prevent it in all instances, I began in September last to use what is called "flat silk elastic," which is a ribbon about a sixth of an inch broad, made of about eight threads of indiarubber, interwoven with silk. One yard of it will tie about a dozen ligatures, one being sufficient in each case. As it is very strong, it can be tied tightly on the cord, which it follows as it shrinks, so that no hæmorrhage can possibly happen. In the specimens shown, the difference between the two kinds is well seen—the one being quite slack, while the other still maintains its gripe.

Dr Bruce said he had rarely, if ever, been troubled with bleeding from the cord, and he was in the habit of using three ply of common thread.

Dr Bell agreed with Dr Bruce, that three or four ply of gray thread formed a perfectly secure ligature. He was afraid that the elastic band, proposed by Dr Dickson, would yield after a little heat and moisture.

Dr Burn preferred a few plies of thread, moistened and twisted; and he was always in the habit of applying two ligatures.

Dr Carmichael acknowledged that Dr Dickson's method was ingenious, but he had always got on well with thread or twisted tape. These latter, too, were always at hand.

Dr Pridie used twisted tape or thread, and found them quite effectual.

Dr Fraser had every confidence in twisted tape, and he preferred it to thread, as this latter might cut through a very gelatinous cord, unless care were exercised. He suspected that heat and moisture would destroy the elasticity of the flat indiarubber silk of Dr Dickson, and prevent it following the shrinking cord.

Dr Cairns regretted that he could not recommend Dr Dickson's invention. An elastic band, unless put at full stretch, will certainly recoil; and if so, the vessels of the funis will, of course, be left open. There was most trouble from the internal or constitutional hæmorrhage, and frequently the cautery and other devices had to be employed. Dr Cairns had invented an instrument which, he believed, would supplant the ligature, viz., an article acting on a swivel, which crushed or rather chewed the vessels, and thus resembled the method of the lower animals.

Dr Duncan thought Dr Dickson's plan both ingenious and efficient. Dr Dickson had exhibited specimens which demonstrated that this elastic band constricted the cord, and bound the vessels more thoroughly than by means of the usual ligatures.

Dr J. M. Hyslop, Edinburgh, was ballotted for and admitted as an Ordinary Fellow.

SESSION XXXIII.—MEETING VI.

Wednesday, 28th January—Dr J. MATTHEWS DUNCAN, President, in the Chair.

I. ON A CASE OF SPURIOUS PREGNANCY WITH LABOUR.

BY DR UNDERHILL.

Early in October of last year, Mrs M., aged 23, came to ask me to attend her in confinement. She had been to consult her medical man, not an accoucheur, who sent her to me, after hearing an account of her symptoms, and telling her she was pregnant. Her history was as follows:—

Previous to marriage, her menses were always regular, painless, and lasted about four days. She was married in January last, menstruated in February as usual, and since then has seen a very small quantity of discharge every month or six weeks, seldom lasting more than a day, or being more than enough to stain one cloth. The discharges were not quite regular, and were sometimes accompanied by pain. In March, April, May, and June, she was sick, and vomited every morning, the sickness passing off about midday. From this time her belly began to swell, so that she several times found it necessary to let out her dresses. She has been feeling the movements of the child for several months, but does not know when she first felt them. Her breasts have also enlarged, but not very much.

I only saw her for a few minutes, and as she had a large waterproof cloak on, I was unable to see anything of her figure, but agreed to attend upon her, with the intention of inquiring into her monthly menstruation when her labour came on.

On Thursday, 13th November, I was sent for in the morning, and found her sitting up. She said she had had pains off and on since the Monday, and had had a considerable "show." She also stated that the pains were coming on now about every ten minutes, had been bad all night, but were better in the morning.

I was sent for again about 2 P.M., and found her lying on the bed undressed, and as I entered the room she was apparently in the middle of a violent expulsive pain. She was crying out lustily, and biting a handkerchief between her teeth to prevent her cries being heard, while at the same time she was pulling hard at a cloth attached to the bedpost by way of helping the pains. I was told that pains of similar severity were coming on every five minutes or oftener. She complained mostly of pain round the back and loins, and some little in her belly. I saw another pain very similar to the first I had seen, and then tried to make a vaginal examination, fully expecting to find labour well advanced. I found great difficulty in introducing my finger, owing to a most determined contraction of the thighs together, and as soon as I touched the vulva, she threw herself round on her back, struggling and shrieking loudly. She declared that the mere touching of the vulva gave her great pain in the back. The sphincter vaginæ was in a condition of spasmodic contraction; but, by determined persistence, I at length managed to reach the os, and, to my surprise, found it quite virginal, small, round, and hard; the pelvic brim was not occupied by any swelling or hardness, but all the parts felt quite natural. In fact, neither pregnancy nor any condition of disease could be made out by the finger. On examining the abdomen, it was found to be moderately distended in its lower part, but not nearly large enough for a full-time pregnancy; it was resonant to percussion all over, but the tapping caused her to wince and cry out as if in pain. I then made gentle pressure towards the pelvic cavity, and while engaging the patient in conversation, pushed in my hand, so that I could feel the promontory of the sacrum, and satisfied myself that the cavity of the pelvis contained no tumour large enough to be felt from the outside. On examining the breasts, I found them full, but not hard, with a light-brown areola, but the nipples were small, and not puffy, or in any way resembling the nipple of pregnancy. I told her at once that she was not pregnant, and that the whole thing was a mistake; and afterwards, though she complained of soreness and pain round the back, she had no more of the imitative throes of labour.

Thinking that there might be some vaginismus from the difficulty I had found in examining her, I asked her, and she stated that connexion gave her great pain occasionally, but not always; and from her husband I learned that connexion was perfect, and, as far as he knew, without any pain to her, and that at least she complained of none.

The next day she was sitting up, the discharge still went on, but beyond a little soreness in the back, she was quite well. A dose of laudanum had given her a quiet night. Such are the facts of this case.

I need not remind the Society of the many cases of spurious pregnancy on record, though spurious labour appears more rare. The peculiarity of this case seems to be, that the patient is a young woman of very good sense, and not at all of an hysterical character; that she was induced to believe in the existence of pregnancy by the opinion of her medical man, somewhat against her own judgment. She had some doubts about the pregnancy all the way through; that this notion had fixed itself in her mind; and that when a more than usually powerful stimulus to the sexual organs had arisen in the form of a delayed and, for her, unusually copious menstrual discharge, this notion had exploded in the pains of a simulated labour. The imitation of the labour pains was very striking, the more so, as I found subsequently that the patient had never seen a woman in labour. In the words of Hamilton, as quoted by Montgomery, "She acquired the most accurate description of the breeding symptoms, and with wonderful facility imagined that she felt every one of them."

I may mention that this occurred in a common stair where there were five or six other young women, who had been married about the same time as my patient, and who all had either been confined or were expecting to be so directly; in fact, there was in that stair a kind of epidemic of babies, and my patient had caught the complaint.

In this, as in most cases of spurious pregnancy, there were many of the usual symptoms of pregnancy, but some startling exceptions—among the latter may be noticed the continuance of the menstrual discharge, though rather irregularly, and in small amount. The abdomen, too, was not much distended—a fact I did not discover until the so-called labour, as I had no opportunity of making an examination previously. Like many similar cases, there was apparently no uterine or ovarian disease.

I saw the patient last week, and she was in perfect health. One lesson, at all events, this case has taught me, and that is, that when a woman thinks she is pregnant, and yet continues to have a tolerably regular discharge every month, however small the amount be, and however different from her usual habit, the existence of pregnancy must be looked upon as extremely doubtful, and the other symptoms must be carefully inquired into.

Dr Macdonald said that Dr Underhill's case was a peculiar one. He could not say that he had met with what might be called a genuine case, although he had encountered many women who fancied they were pregnant, and were not. Such cases were not unfrequent; he might add, they were interesting and even inexplicable. It was impossible to explain the relation between mind and matter here—in other words, the mental processes leading to all the physical phenomena of labour.

Dr Gordon said there must be something more than fancy in such cases, for the lower animals were liable to the delusions of pregnancy and labour—that was, of being pregnant and parturient when they were in reality not.

Dr James Young said he had been engaged to attend a lady where menstruation had ceased for nine months; the belly was distended to the full size. Fœtal movements were felt, according to the patient's views. The nurse and baby-linen were all procured. Dr Young was sent for hurriedly to the delivery; but, on examination, the whole distended belly collapsed in a moment, air being the only product.

Dr Duncan had seen many cases of spurious pregnancy, but only one of spurious labour. This was a lady who had borne five children previously. The menses had stopped, or rather, there was a scanty discharge, and at the wrong time. The abdomen was greatly distended. He received a pressing message to go to the lady, but being out of the way, a second most pressing one came. His diagnosis was, on examination, that the whole thing was a mistake—there was no pregnancy. The lady was quite incredulous. She was no novice, having borne five children previously; she knew the symptoms well. Still, as before said, there was no genuine pregnancy—no living product. This lady, Dr Duncan added, kept up the delusion well, and made her friends believe that she had really given birth to a child, but that it was stillborn. Dr Underhill might have entered more profoundly into the interesting subject—and it was really interesting. There were various degrees of it. For example, there were some who merely exhibited the symptoms of pregnancy, and these symptoms never culminated in spurious labour. Were it not for the undoubted fact that some of the lower animals, such as bitches, exhibited spurious parturition, he would be inclined to deny it altogether. As it was, the thing could not be challenged—it was no mistake or delusion—at least, barring a few cases; but a reality, and, in fact, a disease. Dr Duncan desired to emphasize that a distinction should be made betwixt those cases where there was merely spurious pregnancy—that was to say, where women fancied themselves pregnant—and those where this fancied pregnancy culminated in a fancied or spurious labour. Authors had not sufficiently attended to this distinction.

Dr Carmichael inquired whether such cases were more prone to occur at or near the period of the final cessation of the menses. He had found it to be so.

Dr Duncan said that the disorder was specially apt to occur at that time.

Dr Wilson said he had not seen many cases of spurious pregnancy; that was to say, where there was also fancied or false parturition. Of course, he had often met with females who thought they were pregnant when in reality they were not. He believed that sheep were liable to the delusion, and, in fact, discharged something, as was stated by Harvey.

II. ON THE RELATION OF THE VOMITING OF PREGNANCY AND THE QUANTITY OF THE LIQUOR AMNII. BY MR DONOVAN. COMMUNICATED BY DR RITCHIE.

In bringing the following case before the Society, I am actuated by a desire of obtaining the opinion of the Fellows, and thereby increasing my own knowledge of the subject.

Some months ago, I was consulted by a lady who was about six months pregnant, and who, almost from the date of conception, had suffered so severely from sickness of the stomach, that her life was a burden to her. She had not been able to retain food of any sort for more than half an hour—in fact, it was no sooner down than it was up again. I ordered her a mixture containing bismuth and hydrocyanic acid, which, when taken before eating, enabled her to retain her food for about two hours. This state of things continued up to the time of her confinement, in which I attended her. When I saw her she was twelve hours in labour. On making an examination, I found the os very slightly dilated, the head presenting, but no trace of the usual bag of liquor amnii. On asking the nurse when the waters had come away, she told me there had been no discharge at all. Certainly there was no sign of water or other loss on the bedclothes. The os uteri showing a tendency to rigidity, I recommended the patient to inhale a small quantity of chloroform, which she refused. She, however, took some warm beef-tea, which she threw up, and in a short time the os relaxed. The first stage of labour had continued eighteen hours. In two hours from the time the os uteri commenced to relax, the patient was delivered of a healthy, fully-developed child, without the loss of more than an ounce of fluid, which appeared more like mucilage than liquor amnii.

The inference I have drawn from this case is, that the distressing symptoms during pregnancy were caused by the almost total absence of liquor amnii allowing the fœtus to come in direct contact (during movement) with hypogastric and other ganglia of the sympathetic nerve situated in the neighbourhood of the uterus.

Dr Wilson said he had seen numerous cases, but would not pretend to advance any theory as regarded causation. He found those cases occurring at the earlier months ameliorated by simple means, such as taking a light breakfast in bed.

Dr Macdonald said that Mr Donovan's theory was a reproduction of one frequently heard, say in the Cowgate, viz., that the hair of the child rubbed against the stomach of the mother, and thus excited vomiting.

Dr Gordon said he had never observed the slightest connexion between the amount of liquor amnii and the sickness of pregnancy.

Dr Duncan said that it was extremely desirable to have some light thrown on the subject of the causation of sickness and vomiting during pregnancy, for it was a serious and painful, and even fatal, disorder in some cases. We had certainly means to soothe it, but treatment was quite unsatisfactory. Referring to Mr Donovan's theory, he remarked that it was not the right one, and this for diverse reasons. Firstly, where there is no liquor amnii there can be no (intrauterine) fœtal movement: the thing is impossible. No child, not even an adult, could move if tied up, so to speak, tightly in a bag or sack. If Mr Donovan is right, then the sickness should be greatest at or about the seventh month, when, as a rule, the child has most facility of movement; but this is found not to be the case. Secondly, in the great majority of cases, the sickness was greatest during the first half of pregnancy, and at this earlier period the movements of the fœtus were weakest. Thirdly, in cases where the liquor amnii had been discharged even for six or eight weeks, such as the one he had related to the Obstetrical Society of London, it had been found that there was no supervention of sickness. Dr Duncan added, that he had seen cases of sickness to a morbid extent from time to time. He had seen one case turn out fatal in the earlier months, and where the pregnancy was not marked by anything else unusual. Finally, Dr Duncan said there was still much mystery attaching to the pathology of the disorder.

Dr Gordon had also seen many cases, and agreed with Dr Wilson as to the benefit of a simple breakfast in bed. He thought the causation still unknown.

Dr Underhill inquired if the disorder ever happened to the lower animals, but none of the Fellows present had met with such cases.

Dr Furley thought that the pressure of the fœtus might have something to do with the vomiting; for this was common during labour, especially when the presenting part pressed on the os.

Dr Milne, though not as yet prepared with a theory, had somewhat modified his views as regarded causation. He was disposed to think that at least the more obstinate cases might be explained by some direct or indirect influence on the central-nerve masses; in others, renal irritation might account for the disorder.

Dr James Williamson, M.B. and C.M., Inveresk, was balloted for, and admitted as an Ordinary Fellow.

Part Fourth.

PERISCOPE.

THE following letter, addressed to the Editor of the *Practitioner*, and published in the February number of that Journal, is so interesting, not only for the information contained in it, but also in its reference to the valuable paper on the subject, published in this Journal in 1866, that we transfer it in full to our Periscope.

ED. *E. M. J.*

THE BROMIDE QUESTION. BY DR WARBURTON BEGBIE.

EDINBURGH, *Jan.* 15, 1874.

Dear Dr Anstie,— I have read with much interest in the *Practitioner* for this month, your translation of the paper by Professor Binz, of Bonn, on "the therapeutic employment of bromide of potassium," and your own account of "the English standpoint respecting the value of bromide of potassium."

Were it not that you have inadvertently fallen into error in ascribing to me the authorship of the article on bromide of potassium which appeared in the *Edinburgh Medical Journal* for December 1866, I should not have troubled you with this letter. My appreciation of the value of bromide of potassium as a remedy in various diseases is, however, so high, that I am jealous of any testimony which has been borne to its therapeutic actions failing to exert the influence which such testimony justly possesses. Permit me therefore to state that the article in question was written by my father, the late Dr Begbie, whose patient investigation of the virtues possessed by bromide of potassium and strong recommendation of its use did much to secure for it the confidence of the profession in this part of the country, and largely contributed to its general popularity.

Having made this correction, I am encouraged by the perusal of what you have written to add my testimony to the value of bromide of potassium in the treatment of various diseases. This I shall do in a categorical manner, in something of the same way as you have recently done.

1. *Epilepsy.*—In this disease my experience of bromide of potassium entirely confirms the statements of Dr Russell Reynolds.

I have repeatedly seen cures, in the strictest sense of the word, result from its employment, after the failure moreover, in many cases, of other remedies. Not only have severe fits ceased to return under its use, but the general health of patients, and more especially their mental condition, which had seriously suffered, have been completely restored.

Years have elapsed in certain instances since the occurrence of a fit, and individuals who had, owing to the frequency and severity of their attacks, been rendered incapacitated for their employment, have been enabled to resume their occupations and continue them without interruption.

Some patients have been benefited but not cured. The fits in such have been rendered less frequent in their occurrence, and less severe, but have not been entirely removed. Still, in these cases the bromide of potassium has been truly the *summum remedium.* No regulation of diet, no peculiar stringency of regimen, no other remedy than bromide of potassium—and many remedies have been tried—has exerted the like beneficial influence. Accordingly, it has in such cases been continued for the purpose, as you and Dr Hughlings Jackson have observed, of reducing the frequency and severity of the fits. Some cases of epilepsy have in my hands been in no respect benefited by the bromide of potassium. A few have apparently been aggravated. I have not been able to satisfy myself of the reason for this varied but exceptional experience. It is, however, a common experience in the use of other admirable remedies. *Nullum medicamentum est idem omnibus.*

Looking back upon my experience of epilepsy, I feel inclined to remark that, were I deprived of the bromide of potassium, I should conclude that my best hope of being useful to the sufferers from this sad disease had been taken away. I cordially embrace the aphoristic deliverance of the authority whom you quote in the concluding sentence of your paper: "It has changed the whole prognostic significance of epileptic attacks."

2. *Insomnia.*—In the procuring of sleep, bromide of potassium may be said to fall far short of opium, chloral, henbane, and other narcotics; and yet in many cases of insomnia it is superior as a remedy to the whole of these. Its innocency is in the first place to be set against their potency, not unmixed as that potency is with injury or even danger.

In the sleeplessness which succeeds mental shock, or is occasioned by long-continued mental strain or by worry, the bromide of potassium in full dose is oftentimes singularly efficacious, not only procuring much-needed sleep, but tranquillizing the whole nervous system, and rendering the individual, otherwise quite unfit, capable of mental exertion.

I have repeatedly prescribed the remedy with the happiest results in cases of insomnia accompanied by general restlessness and incapacity for exertion, consequent upon long-sustained mental effort with anxiety in professional men, and on prolonged devotion to business in persons following different kinds of mercantile pursuits, in whom rest, change of air and scene, the most careful attention to diet and regimen, including treatment in hydropathic establishments, and the use of other drugs, had entirely failed to produce any good result. I do not affirm that the bromide of potassium always succeeds, or that it has always succeeded in such cases. I entirely concur in your observation that the insomnia of aged persons is apt to be aggravated by the bromide of potassium, although I have not found it to be always so, as your experience appears to have been. In one case of an old lady the remedy certainly did harm. She, however, had notable calcareous degeneration of the arterial tissues; and from my observation in her case, and in other old persons, I have been led to surmise that the condition in question interferes with the physiological action of the salt, and with its therapeutic action likewise. Bromide of potassium is believed to contract the minute vessels, and if degeneration of their walls exists to a marked degree, in failing to produce this effect it is possible that the presence of the salt in the blood may excite cerebral disturbance in place of quelling it. Whether this theory be correct or not, I have for a considerable time avoided the use of bromide of potassium in old people whose vessels were evidently the seat of general atheromatous degeneration, but have prescribed it in the insomnia of the aged when this morbid condition of the vascular system was not conspicuous.

A further and most important use of bromide of potassium is as an adjunct to chloral. I have found twenty grains of the former greatly increase the efficiency of a like dose of the latter.

The insomnia of delirium tremens is often overcome by large and frequently repeated doses of bromide of potassium, and so also is the sleepless excitement of puerperal mania. In these maladies the combination of bromide of potassium and chloral is chiefly to be recommended.

I have had occasion to verify the important observation of Dr Begbie, that the craving for alcoholic stimulants which is so distressing a feature of dipsomania, is to a certain extent, even in some bad cases, and to a much greater degree in the milder, restrained by bromide of potassium.

3. *Spasmodic Diseases.*—In controlling habitual cramps of the lower extremities, I have found no remedy so useful as bromide of potassium; and undoubtedly the very distressing cramps of the formidable Asiatic cholera were found during the last prevalence of that disease to be subject to its influence.

In spasmodic asthma I have had a considerable experience of bromide of potassium, and have been led to rank its virtues very highly. In a review of the late lamented Dr Hyde Salter's excellent work on asthma, undertaken at the request of my friend Dr Sanders, then editor (1869) of the *Edinburgh Medical Journal*, I took occasion to express a favourable opinion of this salt as a remedy in asthma, and, at the same time, surprise that it had not even been named by Dr Salter.

Perhaps in the treatment of asthma no remedy has appeared to me so useful as the iodide of potassium, but in my experience bromide of potassium has effected a cure when the iodide has failed. The union of these two salts, and their combination with arsenic, has been still more efficacious.

4. *In the incontinence of urine of young children*, bromide of potassium has answered when even belladonna had not succeeded, and these two remedies are probably the most available in this often troublesome disorder.

A less experience of its use in the following diseases has led me to the conclusion that in each of them the bromide of potassium is a remedy well deserving trial:—Hysteria, more especially its convulsive forms (in these Sir Charles Locock had reliable proof of its value), gonorrhœa, and certain non-malignant enlargements of the liver and spleen, the former more especially when connected with the too free use of alcoholic drinks.

I refrain from mentioning the diseases in which the use of bromide of potassinm has been followed by results either negative or wholly unsatisfactory. Let me, however, state that I have grave doubts of its being, in the strict sense of the term, a febrifuge. In relieving the restlessness and insomnia of the febrile state, it unquestionably does good: but such therapeutic action does not entitle bromide of potassium to rank as an antipyretic.

I agree to the fullest extent in your judgment of the value of such articles as that of Professor Binz, in forcing us to scrutinize our grounds of belief in the action of remedies with additional rigour. Although I regard Professor Binz as essentially wrong, it will be my duty, after reading his paper, to reconsider the position I have been led to assume.—Believe me, dear Dr Anstie, yours sincerely,

J. W. BEGBIE.

PENETRATING GUNSHOT WOUND OF THE PELVIS, with Perforation of the Bladder and Rectum. Recovery. By MM. GEOFFROY and DELAISSEMENT. Société Médicale de l'Aisne.—On the 6th September 1870, while in camp near Paris, a garde-mobile. named Caillaux Pierre, was struck on the right groin by the bullet from a Chassepot rifle, accidentally discharged by a comrade at less than two yards' distance. The ball passed above Poupart's ligament, through the bladder and rectum, and emerged in the left buttock, near the anus. For some days the patient suffered from peritonitis, and was unable to retain either water or fæces. The water passed entirely by the wound in the buttock; the greater part of the fæces also escaped by the same channel. For some days he was in danger, but was able to sit up within a fortnight, and three weeks after the accident he was removed to his own home. At that time fæces and water flowed continuously from the wound. The original channel of the urethra was re-established with difficulty, and gradually the wounds closed; the water then passed almost entirely by the urethra; a small quantity by the anus. Six months after the injury he resumed his occupation as a hairdresser, and he then had complete command over both rectum and bladder, and the only drawback to perfect health was the passage of a few drops of urine by the anus each time he makes water. The patient has since married, and is now in robust health. He says that he performs the duties of a husband very well, but he adds, "L'éjaculation n'a pas lieu par le canal de l'urèthra. Le sperme sort par l'anus."—*La France Médicale*, 27th December 1873.

EXTIRPATION OF THE LOWER END OF THE RECTUM.—These cases are sufficiently rare to deserve notice, especially as the method is somewhat new. A female, æt. 47, had suffered from cancer for two and a half years. The sphincter had lost its power, the patient was losing blood, gradually became feeble and thin, and unable for her usual employment. Patient being put under chloroform, M. Verneuil (1) traced on the skin, by the galvano-cautery, a horseshoe-shaped incision, the convexity of which passed between the anus and the commissure of the vulva, the two ends of which terminated at a line opposite the tip of the coccyx. (2) He united these two ends by an incision which passed a line or two behind the anus, and thus raised what may be called a coccygeal flap, and thus detached it from the deeper parts. (3) He, with his index finger in the bowel and the thumb behind it, dissected, by gradual strokes of a galvano-caustic knife, the parts behind, in front, and at both sides of the bowel, an assistant always pulling the part to a side when the knife was applied. In this dissection the middle hæmorrhoidal artery bled a little and required ligature, but the dissection was done as easily as with a bistoury. (4) The upper limits of the tumour being felt, the rectum was then opened to that extent in a longitudinal direction by the ecraseur. It was then easy to open out the bowel, and have under the eye the upper limits of the disease. (5) In four applications of the chain, M. Verneuil then divided the upper attachments of the disease, which thus was completely detached to the extent of nearly four inches. The wound was then dressed simply with charpie, muslin, and a little carbolic acid.—Dr Nepveu in *Gazette Médicale de Paris*, No. 1, 1874.

CASE OF ARTERIO-VENOUS ANEURISM OF THE ARCH OF THE AORTA, the Cavity of which communicated with the left Brachiocephalic Vein. By M. CHABOUD.—S., æt. 56, came under Dr Laure's care in November 1872. No special hereditary predisposition; good general health and vigorous constitution. Up to May 1872 his general health was excellent; no cough nor oppression of breathing. One morning patient awoke with a severe pain in the left shoulder, which next day extended to the arm, and disappeared on the following day, when the patient, looking in the glass, noticed that the left side of his neck and the lower half of the face were swollen, and little by little the swelling extended to the shoulder and affected the arm. This, however, disappeared entirely in fifteen days. A week later, the jaw, neck, shoulders, and arm again became enlarged, as also the left half of the thorax, before and behind. A little oppression resulted, but no pain. This œdema came and went, but always its disappearance became less perfect. In August the patient noticed that the veins of the left side were swollen, and the skin assumed a violet tint, while oppression of breathing became more severe. In September the symptoms increased, and the arm and hand became swollen. One day in October the patient, putting his hand on the swelling in the neck, noticed, for the first time, a sensation like the purring of a cat. No treatment had been adopted. He entered hospital in November. The cheek, left half of neck, shoulder, and arm, were œdematous, and the chest of same side, as far down as nipple. The forearm was covered by an eruption like acne; the skin was livid. The lesser veins under the skin were prominent and well marked. The large veins of neck and chest were twisted, swollen, and varicose. In some parts they formed plexuses of dark-blue colour, projecting like tumours. Two especially, one over each sterno-clavicular articulation, were soft and reducible on pressure. The hand felt a considerable thrill in these tumours, especially in the left one. They were united by a large vein. In the regions both above and below the clavicle, the purring thrill was still more intense on pressure. Percussion signs were rendered doubtful by the œdema of the chest. A systolic *bruit de souffle* was extremely loud. The murmur was prolonged even to the second sound. It was strongest over the base. Sounds were clear, and with no murmur at the apex. Over the varicose tumours the murmur was continuous, but strengthened at the systole. Many other symptoms are detailed with much minuteness; the chief are:—Weak pulse in *right* arm; dulness of chest on left side, both before and behind, with other evidences of condensation of lungs; slight expectoration, no cough, good appetite and digestion; habitual constipation; face pale, with livid lips; no weakness nor emaciation; no pain, but only oppression of breathing, brought on by walking or standing, or even lying on his back or on his right side. The diagnosis of "an aneurism of the arch of the aorta, communicating with the left innominate vein," was made. Iodide of potassium and digitalis were given. During the next three months the swelling and difficulty of breathing rapidly increased, with loss of speech and weakness, and he died on 22d January.

Post-mortem.—Condensed and atrophied lungs, with pleuritic effusion. Heart was hypertrophied and atheromatous. About an inch and a half from the region of the aorta a tumour, as large as a small orange, was present. From this opened the innominate artery and the left innominate vein. The vena cava superior was to the right of the tumour, and had no communication with it, nor with the left innominate vein. This was not merely from the presence of a clot, but the coats of the vessels were perfectly united. —*Lyon Médical*, 21st December 1873.

Part Fifth.

MEDICAL NEWS.

A New French Journal.—The *Journal de Thérapeutique* has just been started, under excellent auspices, with Professor Gubler at its head, aided by MM. A. Bordier and Ernest Labbée. It is published by Masson, and appears twice monthly. The first number contains an introduction by M. Gubler, and the first part of an elaborate original article by M. Martin-Damourette, on Antagonism and Tolerance—the antagonism of eserine and atropine—and on the employment of eserine against tetanus. In this, though differing in some of his results from those of Dr Fraser, the author alludes to the importance and value of Dr Fraser's experiments and researches. M. Bordier commences an elaborate review of the therapeutic novelties of the past year. We wish the new journal every success.

Death of the Siamese Twins.—On 15th January, Chang and Eng drove from Chang's house to Eng's—one mile. It was cold; Chang had had a cold previously, and also had been paralyzed some time before. They went to bed as usual, and Chang died quietly through the night, apparently without Eng's knowledge; when he knew, he at once became much alarmed, and died in two hours. It is said that their wives have demanded a very large sum for the privilege of a post-mortem examination, and that Dr Pancoast and others have gone to the South for the purpose. Both twins were comfortably situated, Chang having, it is said, left property to the amount of £7000, and Eng to about half that sum.

We learn, from a telegram dated New York, 20th February, that the post-mortem examination has been commenced, and that a peritoneal connexion extended into the band. Fluid injected into the mesenteric vessels of the one passed into those of the other. The liver in Chang was in normal position; but in Eng it was on the left side, and the spleen on the right. There was a common umbilicus, situated in the middle of the band; hence any attempt at separation during life would have proved fatal.

The Riberi Prize.—The following are the conditions of the fifth competition for the Riberi prize of 20,000 lire (£800); the subject of which is "the Pathology of the Female Generative Organs." 1. Complete treatises and monographs are admitted to competition; 2. The works, written in legible characters or printed, must be presented in the Italian, Latin, or French language, and will become the property of the Royal Academy of Medicine of Turin; 3. Printed works must have been published in 1874, 1875, or 1876; two copies must be sent post free; 4. The essays in competition, written or printed, must be sent to the Royal Academy of Medicine in Turin, before 31st December 1876.—*London Medical Record.*

ARMY MEDICAL SERVICE.—The following is a list of candidates (in order of merit) for the Army Medical Service who were successful at the competitive examinations held at London in August 1873, and at Netley in February 1874, after having passed through a course at the Army Medical School at Netley. (Maximum number of marks, 6900.)

	No. of Marks.		No of Marks.
1. Langridge, G. T.[1]	5243	6. Gubbins, W. L.	4370
2. Fowler, B. W.	5209	7. Thomsett, R. G.	4190
3. Webb, W. E.	4690	8. M'Quaid, P. J.	4173
4. Mapleton, R. W.	4650	9. Spencer, F. H.	3836
5. Wood, O. G.	4375	10. Ring, J.	3561

[1] Gained the Herbert Prize.

NAVAL MEDICAL SERVICE.—The following is a list of naval medical candidates (in order of merit) who were successful in the competitive examinations held at London in August 1873, and at Netley in February 1874, after having passed through a course at the Army Medical School at Netley, and who will receive commissions as surgeons in the Royal Navy. (Maximum number of marks, 6900.)

	No. of Marks.		No. of Marks.
1. Hewett, F. C. C.	4317	4. Cooke, G.	3609
2. Preston, T. J.	4173	5. Pearson, W.	3554
3. Bedford, R.	3881	6. Vasey, C. L.	3140

LIST of CANDIDATES for H.M.'s INDIAN MEDICAL SERVICE, who were successful at the competitive examinations held at London in August 1873, and at Netley in February 1874, after having passed through a course at the Army Medical School, Netley.

Order of Merit.	Names.	Number of Marks. Max. 6900.	Order of Merit.	Names.	Number of Marks. Max. 6900.
1.	Wilcocks, A. J.,	5215	7.	Collis, J. G.,	4284
2.	Moloney, T.,	4671	8.	Wilkins, T. J. H.,	3955
3.	Deakin, C. W. S.,	4544	9.	Bartholomens, Z. M. L.,	3510
4.	M'Kay, H. K.,	4505	10.	Thompson, P.,	3105
5.	Swaine, F. R.,	4390	11.	Ross, R. E.,	3057
6.	Allison, H.,	4297			

THERE could be no greater mistake than to suppose that the surgical reputation of Edinburgh is on the wane. It is true that a few operations of acknowledged utility are called after their famed originator, the late Mr Syme. But the sum total of original surgery produced in Edinburgh is probably greater now than at any previous period, and, startling as it may seem to some of our readers, is probably greater than in any other metropolis. The present state of surgical literature gives no just reflex of the labours of Spence, Lister, Patrick Heron Watson, Bell, and Annandale. For independence of view, real work, thoughtfulness, originality in conception, and boldness in action, these gentlemen rank second to no body of living surgeons. Yet much of the product of these scientific labourers is to be found in short paragraphs in the last pages of the *Edinburgh Medical Journal*—pages devoted to the proceedings of the Edinburgh medical societies. The name of Patrick Heron Watson is held in the highest regard by all surgical inquirers. Surgery owes much to his rare abilities. When he speaks we all listen, and know he has something to say. Quite recently he has made known a method of operating on the elbow in cases of anchylosis, of great ingenuity and utility. Some critics have said, " Why operate for mere anchylosis when all disease has passed away?" But " let alone " surgery has never been popular in Edinburgh, and those who are familiar with the surgery of that city, need not be told that anchylosis of the elbow is there regarded as an *un*-necessary evil.—*Birmingham Medical Review*, 1st Jan.

DOCTORS AT FUNERALS.—A medical correspondent of the *Manchester Guardian* calls attention to the serious inconvenience suffered by members of the medical profession, in being expected to attend the funerals of deceased patients. Often, no doubt, the invitation is merely a matter of routine on the part of the undertaker; but sometimes the doctor is bidden by mourning relatives, because he has been the best friend of the deceased and of his family during a long and tedious illness, and they would fear to hurt his feelings by omitting him from the ceremony. In the former case, probably no one who is otherwise professionally engaged scruples to absent himself, or perhaps makes his carriage do duty for himself; but in the latter case, no man of feeling can well refuse to join the train of mourners, even at great inconvenience to himself and possibly to others. The salutary practice of the extra-mural interment has done much to add to the troubles of doctors in this matter, for whilst in country places the churchyard of the neighbouring village is still " God's acre," in London and other large cities the distance to the suburban cemetery is often great, and many valuable hours are taken up in going to, and returning from, the ceremony. The feeling of modern times is, we are happy to know, in favour of diminishing rather than increasing the pomp and cost of funerals. Dickens never did a better public service than when he satirized the folly of modern funerals by the mouth of the "Raven" of *Household Words*, and he set a good example by the directions he left for his own funeral. His description, too, of old Chuzzlewit's interment, with the doctor who "looked as distant and unconscious as if he had heard and read of undertakers, and had passed their shops, but had never before been brought into communication with one," is a fine piece of satire, not undeserved possibly at the time it was written. We believe that all doctors of the present day hold attendance at a funeral to be an unmitigated bore, and it would be as well, therefore, that the public generally should understand, that the absence of an invitation to take part in the last obsequies of a patient would not be regarded as a slight. Presentation scarves and hat-bands are, we hope, quite out of fashion, but they used to offer a means of showing attention to the medical man without trespassing upon his time — a result less satisfactorily gained, perhaps, by the modern undertaker's card *in memoriam*.—*Lancet*.

FISSURES IN ANO TREATED WITH IODOFORM.—Dr Francesco Parona (*Giornale Italiano delle Mal. Ven. e della Pelle*, October), gives his experience of iodoform in fissures of the anus, and strongly recommends it. He believes it acts in great measure as a local anæsthetic, "which allays the spasm of the sphincter during defæcation, while it favours cicatrization by neutralizing the irritating effects of fæcal matters which remain on the ulcerated surface." It has also a direct healing action. Dr Parona uses the iodoform as an ointment (one part to three of lard), and applies it on a small cylinder of charpie, of a size requiring but little force for its introduction. The charpie has the advantage that its filaments adapt themselves readily to the slight irregularities of the anal mucous membrane. The dressing is changed twice a day, and replaced after each motion, and in the majority of cases the pain and spasm caused by the fissure cease in a few days, and the patient is well in a relatively short time. Of four cases, of which details are given, the longest time was twenty days, while one of the patients had been ill four months previous to the treatment.—*Med. Times and Gazette*, 27th Dec. 1873.

EPILEPSY INDUCED BY A FOREIGN SUBSTANCE IN THE EAR.—Mr J., aged 22, while cleaning his ear with a broken piece of needle, lost his hold of it, and it escaped into the meatus out of sight. After an ineffectual attempt to remove it, he abandoned the effort, and felt no further inconvenience from it for several months, except an occasional slight pain in the region of the ear. He was then suddenly seized with a severe paroxysm of epilepsy, succeeded by several subsequent attacks. He, however, recovered from these attacks under the influence of bromide of potassium and the nitrate of silver. Two months later the broken piece of needle made its appearance through the skin on the upper and back part of the neck, about opposite the middle of the inferior curved line on the occipital bone. It was easily removed by the forceps, and was embedded deeply in the tissues, in a direction vertical to the surface. It was a little over an inch in length, and is now in my possession. The fact that it was carelessly dropped into the ear is corroborated by the testimony of several witnesses who assisted in the effort to extricate it. The young man has never had an attack of epilepsy since—a period of two years—and had never had one previous to the accident. His hearing is not notably impaired. What route the broken needle took, and in what manner it induced epilepsy, is an interesting subject for speculation. It is also quite remarkable that the organ of hearing was left unimpaired.—*Philadelphia Med. and Surg. Reporter*, 13th Dec. 1873.

ENORMOUS CALCULUS.—Dr Samuel Gee, at the Medical and Chirurgical Society, gave the history of a case of calculous disease of both kidneys in a man who had suffered from gout. In the pelvis of the right kidney there were found—(1) a great stone, weighing thirty-six ounces and a quarter; (2) eight smaller stones, each larger than a cherry-stone, weighing from nineteen grains to fifty grains; (3) about a thousand flattened stones, like melon-seeds in shape; (4) a large quantity of gravel. In the pelvis of the left kidney was found a stone weighing nine ounces and three-quarters. The chemical composition of the greater part of the large stones, and of the smaller stones and the grit, was phosphate of magnesia and ammonia, nearly pure. The large stone had a nucleus of oxalate of lime, phosphate of magnesia, carbonate of lime, and uric acid.

CORRESPONDENCE.

ANIMAL LOCOMOTION, OR WALKING, SWIMMING, AND FLYING. BY DR J. BELL PETTIGREW.

To the Editor of the Edinburgh Medical Journal.

SIR,—Your notice of my work on "Animal Locomotion," appears to me in many points so erroneous and unfair, that, contrary to the usual custom, I venture to ask from you space for a few observations upon it.

Your reviewer holds up Helmholtz as a model, and certainly Helmholtz is one of whom nobody can be ashamed, for he is (Sir William Thomson, perhaps, excepted) the greatest mathematician and physicist living.

Different investigators, however, adopt different methods.

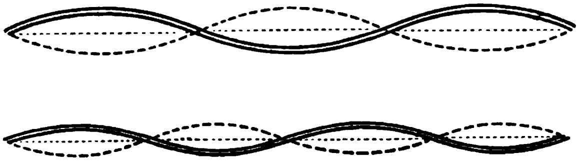
The physicists and mathematicians have hitherto done little for flight.

I venture to think that I have contributed some new facts to the science of aërostation.

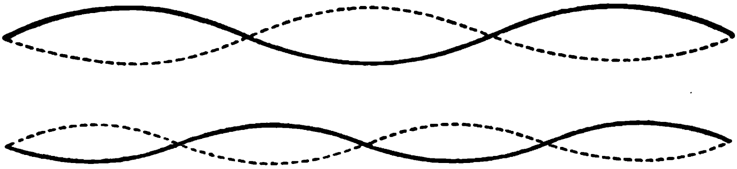
I am told that my comparison of the waves made by the wing in flight with the waves of sound is *utterly erroneous.*

It, however, happens that my diagrams illustrating the waves made by the vibrating wing are identical with those given by Helmholtz of a vibrating cord, *the forms assumed by which*, Helmholtz states, "may also be considered like those of aërial undulations as compounded of simple waves."

The diagrams will speak for themselves.



Pettigrew.



Helmholtz.

Your reviewer takes exception to my figure of speech when describing the extreme rapidity of flight.

It may interest him to know, that the dart of some of the humming-birds is so rapid that the birds completely elude observation when flitting from one tree to another. The path of a cannon-ball in the air is not invisible.

Your reviewer, it may be broadly stated, conveys to your readers no idea whatever of the volume criticized.

The quotations introduced (notwithstanding his assertion to the contrary) are garbled and obscured by being separated from the context.

A work so treated invariably suffers.

I am asked, for example, what on earth is meant by a diagonal twisting movement?

The answer is not difficult.

When a quadruped walks, the right fore and the left hind leg are lifted and advanced together the one instant; the left fore and the right hind leg being lifted and advanced together the next. These movements are universally recognised in physiology as diagonal movements. The bones of the extremities of quadrupeds, however, form screws, the joints for the most part being spiral in their nature.

The muscles, moreover, which clothe the bones are spirally arranged. When, therefore, the muscles contract, as in walking, the extremities twist to a certain extent. It is in this sense that I employ the phrase, "*twisting diagonal movement.*"

The following passages offend your reviewer:—"The boasted levity of insects, bats, and birds, concerning which so much has been written by authors in their attempts to explain flight, *is delusive in the highest degree.* . . . . The space mapped out by the wing during its vibrations is entirely occupied by it; *i.e.*, the wing (such is its speed) is in every portion of the space at nearly the same instant, the space representing *what is practically a solid basis of support.*"

These passages, even when detached from their connexions, are, I venture to hope, not altogether ridiculous.

Until lately, authors believed that flight was in some measure due to the fact that birds are provided with hollow bones and air-sacs. I showed that bats have neither the one nor the other, and yet can fly perfectly, from which it follows that levity is not a *sine quâ non* in flight. Indeed, the converse is true—weight being necessary to flight.

The spokes of a wheel in rapid motion may be said to occupy the space within the circumference of the wheel; for if a body be presented to the wheel, it is instantly struck by one or other of the spokes. It is the same with the rapidly vibrating wing, and thus it is that the space mapped out by the wing in flight represents "*what is practically a solid basis of support.*"

The wings must derive support from the air, and this is the manner in which the support is obtained.

Other passages have been treated in the same carping spirit, not a few having been condemned without the honour of a citation.

Your readers are given to understand that I have, in some way or other, behaved unhandsomely to Professor Marey. The following are the facts.

My theory of flight, which is nearly, if not identical, with that of Professor Marey, was published nearly two years before his. Conscious of the fact, I wrote the French Academy of Sciences, claiming priority. Professor Marey frankly admitted this claim, as the following extract from his letter, addressed to the Academy, in reply to mine, will show:—

"J'ai constaté qu'effectivement M. Pettigrew a vu avant moi, et représenté dans son Mémoire, la forme en 8 du parcours de l'aile de l'insecte: que la méthode optique à laquelle j'avais recours est à peu près identique à la sienne. . . . Je m'empresse de satisfaire à cette demande légitime, et je laisse entièrement la priorité sur moi, à M. Pettigrew relativement à la question ainsi restreinte."—*Comptes Rendus*, p. 1093, 16th May 1870.

Your reviewer concludes as follows:—

"The gist of what Dr Pettigrew evidently regards as his greatest claim to discovery is 'the figure-of-8 wave movement.' An observation made by him, and a little later by Marey, by means of a method long ago used by Dr Young, shows that each point of the wing of an insect (whose body is fixed) describes a curve somewhat resembling the figure of 8. He endeavours to extend this throughout the animal kingdom; and his mode of doing so, as regards the horse, bull, etc., is particularly rich. He seems to have no notion that a part of an elongated figure of 8, *because it contains a point of inflexion*, secures the nearest approach to straightness which can be made by a necessarily curved line. We would recommend him, before he writes any more on the subject, to study Watt's *Parallel Motion*. Here he will find his figure of 8, and may perhaps get some notion of the reasons for its occurrence."

I beg to state that my theory of "the figure-of-8 looped and waved movements" made by the wing in flight, is not based upon *one observation*, but upon hundreds; that, moreover, I used no method other than fixing the body of the insect in a ray of light, and causing the wing to vibrate against a dark background. The method, therefore, adopted by Marey and Young was not resorted to by me. The figure-of-8 movements, made by the extremities of the horse and ox in walking, etc., can be seen without difficulty if the spectator places himself in front of the animals, more especially when they are descending a hill. They can also be seen from behind.

I have long been familiar with Watt's beautiful *Parallel Motion*, but fail to perceive the connexion indicated by your reviewer. By employing a system of jointed rods, Watt rendered the motion of the piston of an engine in a straight line, compatible with that of the beam of an engine in the arc of a circle.

The discovery of Watt's parallel motion in machinery does not necessarily invalidate a similar discovery in animal mechanics, even allowing the two discoveries to be identical. The identity, however, is yet to be proved. If Watt's parallel motion be shown at some future period to be identical with the figure-of-8 movements made by a fish when swimming, a quadruped when walking, and a bird when flying, no doubt some querulous physicist or mathematician will accuse that immortal and deeply-cherished philosopher of plagiarism. J. BELL PETTIGREW.

ROYAL COLLEGE OF SURGEONS OF EDINBURGH,
12*th February* 1874.

[Dr Pettigrew's lengthy remonstrance calls for a word or two in reply. We said *all we could* (perhaps more than we ought to have said) *in favour of his volume*. And what we did so say was, that his illustrations show him to be a good dissector and an able draughtsman; both highly creditable accomplishments, though totally unfit, by themselves, to qualify any man for such a task as this. But we also said that "his comments on, or explanations of," these illustrations, "are, from their excessive looseness, simply bewildering." In this we stated the simple truth, and have nothing to retract or to modify; for it is a fact, however unpalatable it may be to Dr Pettigrew, that his illustrations are valuable and his text hopelessly inaccurate.

We alluded to Helmholtz solely to indicate to Dr Pettigrew how very small a part, of the *necessary* qualifications for such an enterprise as that he has undertaken, is mere skill (however admirable) in dissecting and drawing. Dr Pettigrew does not yet appear to see our meaning; and how should he, when he repeats and attempts to defend several of the absurdities we pointed out in his notions of elementary physics? His present allusion to Helmholtz's diagrams is perhaps more utterly astonishing than anything in his book:—but it shows conclusively that he does not understand Helmholtz. It is totally useless to attempt to argue with one in that unfortunate position. What else could be expected from an author whose arithmetic admits of only *twelve* square inches to the square foot, while confounding three *square feet* with three *feet square;* and who, ignoring Newton's second law of motion, ascribes the flight of a bird to forces (we need not say wholly imaginary) which, if they did act, would *necessarily propel it tail foremost?* These things, and many others equally preposterous, we merely alluded to in general terms. Dr Pettigrew resents our not having given them the "honour of citation." We were merciful, but the Cambridge men have got hold of them, and are making merry over them in the pages of *Nature.*

The Marey question we leave to other hands. Dr Pettigrew has not yet heard the last of it. He makes, however, the important admission that he did not, like Marey, use the exquisitely simple and effective method proposed by Young.

The "discovery (*sic*) of Watt's parallel motion in machinery" is, no doubt, due to Watt; but we are happy to find that Dr Pettigrew makes no claim to what he will probably designate our *invention* of it in "Animal Locomotion." It is quite inexcusable in Dr Pettigrew not to know that Watt's parallel motion depends entirely upon an elongated figure of 8.

To conclude. We have now no hope that Dr Pettigrew is capable of being made to see how utterly erroneous are his pet theories. The adulations of non-scientific journals (due, in part, to his illustrations and the popular interest felt in his subject, but also in a high degree to the vagueness of his text) seem to have blinded him to the fact that all competent judges treat his work with deserved ridicule, or at best with pity.—*The Reviewer.*]

DR KILGOUR of Aberdeen, one of the shrewdest physicians that ever taught clinical medicine, and won the confidence of a province, died at his country residence at Loirston, near Aberdeen, on 19th February, after a long and painful illness. He will be much regretted and long remembered.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On certain Organic Disorders and Defects of Memory.* By THOMAS LAYCOCK, M.D., etc., Physician in Ordinary to the Queen for Scotland, and Professor of Medicine and the Practice of Physic in the University of Edinburgh.

I PROPOSE to illustrate by cases, certain defects and disorders of memory, due to a class of organic conditions not heretofore considered, with a view to a better understanding of their causes and treatment. It is necessary to premise some explanatory observations, so little being known practically of the nature of organic memory and of its relations to mental states. The metaphysician who follows the speculative method either denies that there is an organic basis of memory, or, admitting the general fact, neglects to inquire into its relations to thought and other mental states. On the other hand, physiologists and pathologists who endeavour to elucidate these relations by adopting the abstract terms used by metaphysicians—such as mind, will, memory, attention, perception, association of ideas—without bearing always in mind that these are physiological conditions, are apt to fall into confusion and error.

I have now for many years past viewed all these mental conditions from their anatomical and physiological aspects, and by extending the doctrine of the reflex function of the spinal cord to the brains (and by brains, I mean the whole encephalon, including cerebellum, pons Varolii, and medulla oblongata), I have been enabled to obtain a better insight into the difficult problems which arise in considering the relations of mind and body. I propose to apply the doctrine of the cerebral reflex function to cases of defective memory.

I must premise one general principle, viz., that in all these inquiries, consciousness of any kind is held to indicate a physiological condition of the encephalon, and is not the cause of these conditions or of any mental state, as was held by Descartes and his followers, and by physiologists who speak of sensation or will as a cause. On the metaphysical side, this is the cardinal fact in the study of mental physiology and pathology according to the laws of reflex action. I will first differentiate the states included under memory.

There are two physiological processes included under the word memory, which are fundamentally distinct. Sir William Hamilton thus defines what memory is metaphysically:—

> "Memory, strictly so denominated, is the power of retaining knowledge in the mind, but out of consciousness; I say, retaining knowledge in the mind, but out of consciousness; for to bring the *retentum* out of memory into consciousness is the function of a totally different faculty. . . . It is not enough that we possess the faculty of acquiring knowledge and of retaining it in the mind, but out of consciousness; we must further be endowed with a faculty of recalling it out of unconsciousness into consciousness,—in short, a reproductive power. This reproductive power is governed by the laws which regulate the succession of our thoughts—the laws, as they are called, of mental association."

All this needs to be converted into scientific and physiological language to render it intelligible. It is enough to say here, that the first organic stage—or the *retentum*—is such a change in the brains somewhere, that an act of reminiscence, remembrance, or recollection can take place at all.

But it is necessary to discriminate between various forms of memory and recollection. The word memory is commonly used to denote both retention and reproduction; consequently, these two fundamental processes are confounded with each other when the word is so used. Then, again, it is as often used to denote the reproductive process alone, because want of this is the only sign of defect in the retentive process; hence, the phrase loss of memory generally means loss of recollection. Again: there may be reproduction as a lower mental process, but no memory in the sense of knowing that what is reproduced denotes what occurred in past time. Aristotle notes this kind of memory, and says that lower animals possess it. By reminiscence he meant the higher faculty of knowing the past, and therefore implying the knowledge of time. This is more especially intellectual memory—the memory of events in orderly suggestion. It would be well, therefore, to restrict the use of the word reminiscence to this higher intellectual process of reproduction, as distinguished from that simpler form of memory possessed by lower animals, in which there is reproduction without the concurrence of abstract ideas as to time. Aristotle makes the interesting observation, that the memory, as thus defined, is the more powerful when the intelligence is the dullest, but reminiscence more vigorous when the intellect is the brightest.

As the word memory is usually understood, it implies that some state of consciousness always coincides with both retention and reproduction, the intervening space of time being named by Hamilton, "Mental Latency" and "Latent Consciousness."[1] Consciousness

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[1] See chap. "On Latent Consciousness" in "Mind and Brain," vol. i. p. 175, for a description of this doctrine in its various aspects.

is a term including very different states of brain; but as to organic memory, it is certain there may be organic reproduction without either reminiscence or recollection—only an associated feeling of pleasure or pain. Whether retention as an organic process can take place with the brain in the state in which there is no consciousness of any kind whatever, is doubtful.

The notion of an organic change with retention, or the primary and most essential part of memory, has always had practical recognition; but abstract terms, such as mind and memory, have been used by metaphysicians to indicate the *locus in quo*. Thus thoughts, events, and things are often described as recorded in—written on—impressed on—stamped on—rooted in—burnt into—the memory; the various phrases being used to indicate intensity of the retentive process. Nor has it been uncommon to substitute brain for abstract notions. In the passage in which Macbeth addresses the physician, and asks for relief, Shakespeare denotes four distinct theories of memory. I quote three of them: "Canst thou not minister to a mind diseased? pluck from the memory a rooted sorrow? raze out the written troubles of the brain?" Any hypothesis of this kind is, however, too materialistic for use. The brain is not and cannot be "written on" or "impressed." What happens is a cerebral vital change, which is the double result of mind or mental energy and vital energy acting conjointly with certain molecular energies, under certain physical conditions of the organic basis. The whole series of processes are vital, and have their analogies in other vital processes.[1] Simply, the mechanism is this: the process of retention takes place in relation, firstly, to some antecedent retention; and, secondly, to some external impressions: the one includes recall, as "association of ideas;" the other, recall in relation to external impressions, as recollection or remembrance. Thus, when a man ties a knot on his handkerchief, in connexion with a certain intention he has fixed in his mind, so that he may be reminded of his intent by the touch of the knot, he receives virtually, when so reminded, a physical impression on or through the sense of touch, which being transmitted onwards to the *locus in quo* in the brain, acts on the organic seat of the retention, and so the organic change coincident with remembrance is excited, and he does what he intended. This process is, in fact, one of those cerebral states which, according to my view, are dependent on the reflex action of the brain. The first stage is an example of cerebral sensory reflex action or association of ideas; the second, of reflex cerebral motor action. In thought without volitional activity, the reflex action is not directly induced by external impressions, but indirectly and by successional waves of change in the sensory vesicular neurine.

It is a well-established fact of experience, that all the processes included under memory and association of ideas are natural and vigorous, according as the nutrition of the brain is healthy and vigorous. The state of nutrition thus suitable to healthy mental activity depends upon various conditions, of which I may mention chiefly four:—1. A supply of healthy blood containing suitable nutrient materials. These may be deficient from exhaustion, either by too great muscular or cerebral work, or from a defective diet or supply of food. 2. A competent blood-distribution, which will depend on the structure and vaso-motor activity of the bloodvessels and capillaries. Incompetence is common in diseases of the blood-vessels, such as occur in general paralysis, senile dementia, and other like conditions. 3. A competent fitness on the part of the organic basis to take up and assimilate nutrient stuff. This often depends upon congenital or hereditary quality of brain, or amount of nutrient or evolutionary energy. And, 4. A due accumulation of the assimilated material, to which the state of sleep is most subservient. This accumulation of material for work in the essential tissue is in accordance with a law which extends throughout all organisms, whether plants or animals. The food stored up with seeds and eggs, the storing up of food for ova when developing, of fat, water, are instances.

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[1] See, for a discussion of this point, "Mind and Brain," vol. ii. chap. i. p. 2, "On the Substratum of Conscient Mind."

Looking, then, at the physiology and pathology of organic memory from this trophic point of view, we can distinctly generalize two different, yet closely-related, processes: the one kind including that organic nutrition of the brain on which depend the well-known mental processes termed remembrance, recollection, recall, reminiscence, and association of ideas, and which are determined according to the laws of sensory reflex action of the brain; the other kind, without a distinct name, but which consists in the production of those states of molecular nutrition of the brain upon which the former depend. As this organic basis of memory is the result of mental, vital, and physical energies operating conjointly, I have elsewhere designated the process by which it is formed *synesis*, or a coming together, and so tried to indicate that conjunction. I subjoin an extract from my published work explanatory of my views as to this process:—

> "All experience proves that memory, whether it be retention or reminiscence, or both, is inseparably dependent on vital changes; but the physiological explanations hitherto given have been wholly unequal to the exigencies of the problem; and, at this moment, advanced physiologists pronounce the phenomena inscrutable and mysterious. But, by taking a more comprehensive grasp of the subject, and looking for the biotic correlatives of these changes [upon which memory is dependent], we find that they can be brought under fundamental laws of life and organization. The conservative or retentive process is manifested in that vital process, whereby the modes of vital energy that are, or have been, realized in time and space whether in plants or in animals, and whether manifested as energies, functions, faculties, or states of consciousness, are potentially conserved and retained in those particles of living matter, out of which a new organism is evolved, whether they be termed germs, pollen, zoids, zoospores, and spermatozoa, or primordial cells. This process not only secures the conservation and retention of the type [or the specific identity,

corresponding to personal identity], but of all those departures from it, induced by the adaptation of the parent organism to new external conditions, so that special hereditary tendencies and new instincts are developed, as well as new species and varieties. In short, it is the process which lies at the foundation of the Lamarckian and Darwinian hypotheses. This most fundamental vital process has received no name, neither has it been considered, so far as I know, in its relation to memory and thought. I propose, therefore, to designate the general process by the term *synesis* (a classic Greek word), and adjectively, *synetic*," etc.[1]

We can now generalize further: The organic cerebral or encephalic process I term synesis, which corresponds to memory, considered as the process which subserves to the retention, conservation, and accumulation of knowledge and experience, being a manifestation of a general law of life, the correlative organic process which corresponds to that element of memory termed recollection, reproduction, and reminiscence, must be also a manifestation of some equally general vital law correlative with synesis. Now, these two laws are, in fact, none other than those termed evolution and reversion. To understand this, it is necessary to remember that the brains go on evolving long after the body has ceased to evolve, and that every addition to a man's experience and knowledge is practically a higher evolution of brains, or of some special portion of them. And this higher evolution, due to ever-recurring synesis in individuals, families, and races, may, and in fact does, become hereditary. So that a return in memory at any moment to that which was evolved synetically, a day, a week, a year, or a lifetime before, is, practically, a reversion to an antecedent evolution. So, in like manner, a return of civilized man to the habits and sentiments of uncivilized races is a reversion to antecedent but ancestral synesis as evolutions. Whether or not there be with the organic or ancestral reversions thus established the knowledge that they belong to the past, and, as such, are true reminiscences, is another question, which I will not now discuss.[2] As to these phenomena, whatever they may be—whether merely reversionary or reminiscent also—the process of recall is one of cerebral reflex action; they are re-excited by external impressions.

It follows from all this, that the organic conditions upon which memory depends have a *locus in quo*, and that there is a portion of the brains in which the processes of synesis or evolution and recall or reversion occur. All organic memories are therefore local. This is, in fact, the principle which underlies the phrenological doctrine of "organs." Gall's attention was first directed to the "organ of language" by observing that persons with good memories had prominent eyes. But, then, these persons have not only a *copia verborum* as to knowledge, but a volubility sometimes amounting to a logorrhœa in expressing what they do know—although that may not be much. Hence there is a motor as well as a sensory or sentient element in the processes to which the "organ" is subservient. In phrenology, memory is the result of activity of each of the organs or faculties; thus, by the the organ of "Time," a man remembers music, as well as language and words. The differences in power of retention of "impressions on the memory" manifested by individuals is recognised by phrenology as a quality of brain, but the cause is not known. It is not difficult, however, to determine three conditions as necessary to the proper functional activity of the organic memory, viz. :—1. Those of nutrient energy of the *locus in quo*, whereby synesis and recall are duly perfected; 2. Those upon which thought and ideation, or the association of ideas, depend, which are sensory; and, 3. Those which subserve to the expression of thought or feeling by words or acts, and which are motor. The pathology of memory must therefore discriminate as to these, both as to causes and seat.

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[1] Mind and Brain; or the Correlations of Consciousness and Organization. 2d edition, vol. i. p. 407, sec. 301.

[2] I refer the reader to "Mind and Brain," 2d edition, vol. i. pp. 404, 411, for facts on this point.

That the seat of the motor and sensory are distinct, may, I think, be learnt by an observation, which, however, each reader must make upon himself. Having selected some song of which both the air and the words have been stored up in his brains, the reader should first try to sing both the air and the words mentally, that is to say, without any motor activity whatever, and he will find that he can do this—unless it be that, perchance, his breathing keeps the time of the air involuntarily. Having done this sufficiently often, let him next end the process by energizing into actual singing. In this way he cannot fail to discover that he has been engaged in two distinct cerebral processes, the one restricted to consciousness, thought, or mind, the other combining motor activity with the mental states. Now, let it be supposed that the former or sensorial brain-state is induced physically or morbidly, and independently of volition and thought, then, in this case, the hallucination of hearing the words or the air, or both words and air, would occur; or, if that cerebral sensorial condition which corresponds to the volitional production of the air and words mentally were induced, he would have the hallucination that he sang them, although perfectly silent. This is, in fact, what occurs in dreaming of acts done. Again, suppose, further, that the motor portion of the synesis—that which corresponds to actual vocalization—be also morbidly excited, then he would repeat the words or sing the song automatically or involuntarily. Neither class is of unfrequent occurrence during sleep and dreaming, in delirium, and in insanity. One day I had an interesting illustration of the dreaming class at the Royal Infirmary. A patient at visit complained that he was ridiculed by the nurse and the other patients. They pretended that he had been singing a Scotch song (which he named, a comic song of domestic life) in the night, but which it was impossible he could have done, as he neither knew the words of the song, nor could he sing the air. Upon inquiry, I found that it was indeed the fact that he had so sung, and loudly. This patient had no special head-symptoms, but he was hypochondriacal, and had a spinal neuralgia, and the singing might have been due to reflex action, originating in the spinal cord. To have this result, the patient either must have previously acquired the corresponding organic conditions by synesis, or else had derived them from his parents. The former is by far the more probable, although the latter is not perhaps absolutely impossible.

These views are of general application. The acquisition of language is in precisely the same order as the acquisition of the power and habit of walking. In both cases external impressions are needed to develop that motor and sensory synesis upon which speech, use of the hands, and direction of the limbs and balancing of the body in locomotion depend. None of us remember how painfully we acquired the facile use of our legs; but if the sensory impressions upon which they depend are no longer transmitted from the plantar surfaces in walking, as occurs in locomotor ataxy, the individual must trust to visual impressions; and failing these, when the eyes are shut, he can no longer resist gravity, and so maintain his equilibrium. So, also, if a person become deaf, he generally becomes defective in his speech; if born deaf, he is mute. If suffering from aphasia, he may be able to read when he cannot express his thoughts, because the visual excitor impressions from the printed or written page reproduce the synesis, although those of audition or from above, and connected with association of ideas, being cut off, he is speechless. I have repeatedly shown this difference in the reflex phenomena of aphasia to my clinical class, as to both audition and vision, and I conclude that to understand its pathology we must thus differentiate the various sources and centres of cerebral reflex action into at least motor and sensory.

What name should be given to the organic condition, the result of synesis, upon which memory in all its forms depends? Many years ago, when inquiring into the reflex functions of the brain, I noticed the homologies between the spinal and cerebral reflex acts, in this, that they were due to some special molecular constitution of the sensory and motor centres—to "laws writ on the nervous path," to use a phrase of Prochaska, as to spinal reflex acts. The phrase "material ideas" has been used by various authors to express the organic basis, but the terms are contradictory, since ideas, in the proper meaning of the term, cannot be material. The word "substance" has been commonly used by metaphysicians to express the immaterial basis of mind, but the word has passed into an abusive use, so as to express something quite the opposite. In truth, these conditions, although material since matter underlies them, are not material in any scientific sense of the term, but vital. The material basis has in fact evolutionary properties as manifestly as the primordial cell of the seeds and the ova of plants and animals. I have therefore designated them substrata, those which belong to states of consciousness being *ideagenic*, and those subservient to volition and muscular action *kinetic*. The evolutionary properties of these substrata are best indicated by the phrase *ideagenic*, inasmuch as new ideas, the result of new acquisitions of knowledge, tend to evolve and develop as certainly as a seed or an egg, so that they are in nowise merely material. I have repeatedly indicated these characteristics—first, in an appendix to my paper on the Reflex Function of the Brain, read to the British Association for the Advancement of Science, at its meeting at York in 1844, and published in the *British and Foreign Medical Review*, January 1845; and next, in my review of Unzer's "Erste Gründe," when I specially pointed out their relations to external impressions, and how they differed from the "material ideas" of metaphysicians and physiologists; observing that these substrata "were due to certain changes in the molecular arrangement or constitution of the neurine induced by frequently recurring impressions communicated from without, and corresponding to these impressions and to none other."[1] The acquisition of a song and a tune is an illustration of this process. In 1855 I again explained the meaning of the term, and showed how the substrata have special evolutional properties:—

> "By the term psychical substrata I do not mean to imply a certain material arrangement of cells or their elements only, but such an arrangement that a fixed order of successional changes or plan of action may be impressed upon them. Thus, every primordial or embryonic cell has its psychal substrata, in virtue of which there is a continuous series of successional changes, in a fixed, predetermined order, and according to a fixed plan [constituting evolution]. So also in the cell-masses (or vesicular neurine), appropriate to special ideas, there are psychal substrata, in virtue of which there is a constant construction of new cells, corresponding to those new states of consciousness comprised under the general phrase *development of ideas*, the ideas being developed, and the new cells constructed, according to a fixed and predetermined law of development (or evolution). The substrata have [like primordial cells] *potential* properties—that is to say, they contain the elements of further and indefinite series of future changes, as well as properties in actual use in relation to the external world."[2]

It is absolutely essential to practical ends that the true nature of the organic basis of memory should be understood to be something more than a physical mechanism, like a muscle, or a merely mechanical arrangement of cells and fibrils, the numbers of which may be counted and reckoned as so many "acquisitions." This is an old anatomical and physiological doctrine, taught by Hartley, Bonnet, Hooke, Haller, and others, and by Akenside poetically, and which has been recently revived by Professor Bain.[3]

Looking at each synesis as the last evolution of the brains at the moment it occurs, we must necessarily connect each successive mnemonic evolution with those which have preceded it. This is the aw of associated continuity, and is that which underlies all the theories of evolution of whatever kind they may be. The descent of man from an ascidian, according to Darwin, is according to the same law whereby cerebral changes occur in associated sequence, so as to constitute personal identity.

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[1] Review of Unzer's Principles of Animal Physiology. British and Foreign Medical Review, July 1847, p. 186.

[2] Further Researches into the Functions of the Brain. By Thos. Laycock, M.D., etc. British and Foreign Medico-Chirurgical Review, July 1855, p. 187.

[3] Mind and Body. 1874, p. 107.

The proof of this is not difficult to follow as to the life of each individual in so far as reminiscence extends to infancy, but during the early period of infantile life the synesis is such that under ordinary states of brain there is no reversion possible to infantile evolution, no reproduction, and no reminiscence; it is only in morbid states that this reversion occurs, as in general paralysis or fevers. But extending the law of evolution and reversion to ancestral phenomena, the law of heredity comes into relation with encephalic synesis, both as to continuity and evolution and reversion. Hence it follows that that part of the brains (if any) which subserves to the production of the sperm-cell and germ-cell (the two microscopic portions of living matter which are the media of continuity, as heredity or parental transmission of mental qualities, and functional activity and development of the brain in general) must be in some practical relation to the nutrition and development of those portions of the brain in which are the seats of organic memory. The concentration of the "potentialities" of organic memory in the sperm-cell and germ-cell is itself a most curious synesis, which I have named *genetic*, just as the production of a primordial cell is the most striking manifestation of reversion to the simplest form of life. The power and activity of organic memory—the intensity of synesis and of hereditary transmission—must therefore have some relation not only to nutrient brain-energy, but to the potential energy of the primordial cell upon which the hereditary transmission of acquired faculties, instincts, and habits depends. There can be no more difficult problem for inquiry than the transmission of ancestral organic memory; but I believe it will never be solved so long as inquiry is turned to the merely mechanical arrangements of the nerve-centres, to the exclusion of vital dynamics. I have elsewhere endeavoured to show that the encephalic synesis upon which organic memory depends and the genetic synesis necessary for its hereditary transmission, are both dependent on energy stored up in the cerebellum, and that the amount of this energy, as influenced by blood-supply and other trophic conditions of the organ, influences in no slight degree the evolution of the faculties and the vigour of memory. Referring to my published views as to these functions of the cerebellum,[1] I will now subjoin illustrative cases.

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[1] Mind and Brain, 2d edition, vol. ii. p. 452.

CASE I.—*Loss of Memory of Events after a Fall; Counter-irritation of the Occiput. Recovery.* (I am chiefly indebted for notes of this case to my friend and former class-assistant, Mr R. Lawson, M.B.)

During the session 1871–72, a stout young man, an engineer of a steamship trading to Constantinople, applied to me at the Infirmary on account of an entire loss of memory. He was accompanied by his wife as his interpreter, and at her request was placed on the out-door list. Her account was, that her husband had, through injuries sustained by falling into the hold of his ship, suffered from an almost complete loss of memory of events subsequent to the accident. The account that had been submitted to her was, that he had fallen into the hold backwards; had struck the back of his head against some hard object; that he remained for some time unconscious, and subsequently forgot almost immediately every transaction engaged in by himself or occurring under his observation.

When he came to Ward III., he had complete use of his limbs, with regard both to sensation and motion. His facial expression was that characteristic of absent-mindedness. He looked at objects without manifesting any sense of appreciation, while, at the same time, the impression suggested to an observer scanning his features was, that he was an intelligent man. His facial muscles were symmetrical in disposition, and there was no strabismus. There was no incontinence or retention of urine, and the bowels acted well and under control. On examining the occiput, a slight want of symmetry in the parts corresponding to the superior curved lines was observed. The prominence of the line upon the right side was more pronounced than that upon the left, but neither the extent of the induration nor the recorded symptoms justified the diagnosis of a recent fracture; while the position of the thickening and the defined anatomical nature of it militated against such a conclusion. There had been no discharge from the ears, nor any other characteristic symptom of fracture of the base.

The account given was, that though bodily, and, as far as execution went, mentally fit for his work as an engineer, his forgetfulness, not only of his orders and his own intentions, but of the identity of the piece of work he was engaged upon, frustrated his efforts.

On immediate examination of his mental state at the time when he was first seen, it was found that the recorded statements were completely confirmed. He could remember events anterior to the accident throughout a course of years; but his recollection of circumstances, even of a strictly personal nature, subsequent to the time of the injury, was blank. So complete was the failure of memory on the occasion of the first visit, that though his wife and he had arrived from Leith shortly before the hospital visit, he could not tell whether they had walked or come by train or by omnibus. He could not remember what he had had for breakfast immediately before starting, and had no idea what day of the week it was, nor what period of the day. He endeavoured, by rational propositions, which he could not follow up, to arrive at a satisfactory answer to each question, and, when he failed, gave a negative reply, accompanied by an intelligent and good-natured smile. His special senses were intact. His speech, like his walking, was slow, but precise. He said what he meant to say, and could read correctly.

His wife was directed to have the back of his head completely shaved, and linimentum iodi was ordered to be regularly applied. At the same time, he was prescribed a mixture of iodide of potass in infusion of quassia.

In a week he was brought back, and his memory for recent occurrences had decidedly improved. He could answer clearly such questions as had been formerly put to him unsatisfactorily. He was ordered to continue the treatment as already directed; and, in about three weeks from the time of his first appearance, he was almost completely cured. He was again directed to persevere with such a modification of the remedies as the circumstances demanded, and to return; but neither his wife nor he came back. I had no opportunity of learning whether, with his improved memory as to synesis, he could also remember late events.

It will be observed that this patient had a peculiar expression of "absent-mindedness." It is not easy to be described, but is very characteristic of the kind of case, and indicates that the physical basis of the faculty termed attention is deficient. But an act of attention is a complex reflex act, and, according to my view, includes the various motor structures which subserve to the use of the senses in the faculty termed perception and apprehension, or that stage of the process in which synesis occurs. If the reader will look at an object, or listen to a sound or to a speaker, and examine what he does physiologically, he will find that he adapts not only his eyes or his ears, or both, in the act of attention, so as to receive impressions well and duly, but combines in the act various muscles, which move the head and sometimes the whole body. So also, even in writing or speaking, the whole body is often co-ordinated.

The act of perception has also its reflex motor phenomena; and these are shown in two ways—first, in the expression of the countenance, as indicative of the act of reminiscence, or of judgment or understanding; and, secondly, in the volitional acts which follow thereupon. It is as to the phenomena of expression of thought or apprehension that there is deficient motor manifestation concurrently with deficient perception and imperfect synesis. The expression of absent-mindedness in the preceding case has its analogy in the blank physiognomy of dementia and idiocy. Again, in the motor act of will (or volition) which accompanies a desire to give effect to a thought, the individual equally combines, by reflex action, numerous muscles to the desired end. Now, according to my theory, the *motor* co-ordinative centres of all these cerebral reflex acts are in the cerebellum, and their sensorial connexions in the sensory ganglia and hemispheres. Hence, in this case, either the sensorial impulses from the hemisphere, or the impressions derived from external things, were too feeble to excite the cerebellum so as to induce a proper act of attention, or else the cerebellum was too feeble to react on the muscles, so that a proper reflex act of attention and the consequent perception and synesis should result. It seemed to me that there was no essential or structural disease of the frontal convolutions, or of their bloodvessels, such as is found in cases of dementia and in certain kinds of aphasia, but that there was defective nutrient energy, and, as a result, imperfect synesis from defective excitation in that portion of the frontal lobes which subserves to the memory of events—"the organ of eventuality" of phrenology. This local defect having no motor relations, must be held to be sensorial, and dependent either upon defective sensory stimulus of attention, or deficient power in the tissue to react upon the stimulus. Thus, synesis being imperfect, there could be no reminiscence as to events. In short, as to the tissue itself, it was in the same condition as in dreaming states.

In attributing this morbid condition to occipital shock, involving the cerebellum and sensory centres, and caused by the fall backwards into the ship's hold, I do not overlook the very important facts, that the hemispheres develop backwards, and that there is a community of blood-supply through the basilar artery between the cerebellum and its sensory ganglia and the occipital and temporo-sphenoidal lobes, and, consequently, that disease of the basilar artery, or its branches, may, and indeed does, influence the nutrition of that portion of the hemispheres, as well as of the cerebellum. Indeed, I am well convinced that that community of vascular area has an important practical significance.[1] I wish, however, to use the simpler cases in illustration, like the preceding. But although the patient recovered so rapidly, and for the time probably completely, mischief of a latent kind might still exist, and progressive trophic defects in the hemispheres might arise subsequently, to end in dementia. In such a case the real termination cannot be known until months, or even years, have elapsed. But a wider area of mischief may result from cerebellar shock, as the following two cases tend to show :—

CASE II.—*Maniacal Excitement after a Fall; on Recovery, Loss of Memory of Events; Counter-irritation of the Occiput, with temporary Beneficial Results; consecutive Dementia and Epilepsy.*

In May 1871, an Irish gentleman, aged about 30, came to me, by the advice of Dr M'Munn of Sligo, accompanied by his wife, to consult me regarding his mental state, but more especially that of his memory. I was informed that he almost immediately forgets where he puts things, what shop he had been at, what he has been reading, etc., and seemed childish in his conduct. This had come on as the sequel to a mental attack, which he had in the autumn previously, when he became very excitable, had grand notions about building a new mansion and extending his affairs, and at last became so unmanageable that it was thought advisable to leave him alone in his own house with a suitable attendant. Under the care of Dr M'Munn, of the Sligo County Asylum, the excitement calmed down, chiefly by the use of chloral, bromide of potassium, and digitalis, and subsequently the iodide of potassium; but the feebleness of memory remained, when in all other respects he seemed restored to health.

The patient was of a nervous sanguine temperament, sharp and somewhat "peppery" in temper, but amiable. He seemed to be a man of superior intelligence, and yet, when asked as to very recent events, where he had been, what doing, etc., an expression of absence of mind came over his countenance, and he appealed to his wife for information. I observed that his *right* pupil was contracted, but his wife was confident this was due to the constant use of an eye-glass to that eye. He complained of neuralgia on the *right* side of the head and at the occiput, and the æsthesiometer indicated diminished sensibility on one side. The heart's action was feeble, but regular. No sufficient cause or causes could be assigned for the mental attack which preceded the loss of memory; but on close inquiry, I discovered that a few weeks previously his horse had fallen with him when hunting, and that he had had the back of his head struck. He was stunned and unconscious for a short time, but subsequently felt nothing amiss; and both he and his wife felt satisfied that the fall had nothing to do with his illness. Being, however, from experience in such cases, of a different opinion, I directed the liniment. iodi. to be freely rubbed over the shaven occiput, and the tincture of digitalis taken thrice daily. The patient, in the meanwhile, took a house on the Clyde, near Dunoon. At first, little improvement was manifested, but by the beginning of July, his memory was so much improved, and he was altogether so much better, that he returned to Ireland, his wife writing me a very grateful letter before leaving Dunoon, and saying that he was "decidedly much better."

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[1] See my Lectures on a Clinical and Vaso-Motor Anatomy of the Brain from a New Point of View.—*Medical Times and Gazette*, Aug. 1871, pp. 211, 269.

This improvement was not, however, to continue. The contracted right pupil, the localized occipital pain and anæsthesia on right side, together with the particular kind of loss of memory of events, too clearly pointed to basilar mischief, which might readily become progressive, both in the cerebellum and its sensory ganglia, and in the frontal lobes or hemispheres. And, accordingly, Dr M'Munn, kindly writing to me lately as to the progress and termination of the case, reports this to have been the melancholy termination, to the effect that our patient died just before Christmas last—"He gradually merged into a state of dementia, with hemiplegia of right side, and frequent epileptic attacks."

Such is the course of numerous cases of epilepsy otherwise arising, of sunstroke, and of the result of injuries to the bones, nerves, and of the face as well as of the head. Even an injury to a *sensory* nerve in the finger may progress upwards to the cord, and thence extend to the cerebellum, on the one hand, and onwards to the hemispheres, inducing palsies, epilepsy, insanities, and dementia in its course.

Undoubtedly a certain predisposition to defective nutrition of the brains is an essential element in the causation of these cases, for the subjects are commonly predisposed hereditarily either to hysteria or epilepsy or insanity, or to waywardness and eccentricity of conduct verging thereon. Drunkenness, or a drunken parentage, sunstroke, syphilis, a previous wound or injury to the head, are also predisposing conditions. I was not able to ascertain much as to the influence of any of this class of causes in the case just detailed, and it may be doubted whether the insanity and dementia were wholly due to the fall. It is, however, a fact of experience, that apparently trivial injuries to the occipital region are so followed by serious results. Often, indeed, it is because the immediate results are so trivial that they do not attract attention, or are not remembered, as in this case.

It will save much circumlocution if a distinctive name be given to this kind of loss of memory. *Amnesia*, the old term, is too general, meaning loss of both retention and reminiscence, or else loss of reminiscence alone, whereas neither states were present in this case. I think it may, therefore, be usefully named *asynesia.* What interested me much was to ascertain whether the asynesia came on before, with, or subsequently to, the mental excitement. The patient evidently remembered the accident, and it was clear that nothing seriously wrong with the memory had been noticed before the excitement. Fearing to hurt his feelings, I made no reference in conversation to the excitement itself, having had the particulars from his wife, but I led the conversation up to it, so that he might speak of it, if he had any remembrance of the attack, and the impression left on my mind was that it was very vague, if he had any. In cases of insanity there are two kinds of synesis. In the one kind, it is so intense that an ineffaceable impression is left on the memory—a hypersynesia—perhaps, of some offence taken, causing an antipathy or a delusion of the acute state, which becomes chronic as a "fixed idea," never to be got rid of. In the other kind, the remembrance of the period of excitement is very much like that of a dream, or there may be no more than is seen in delirium, so that the synesis is either very imperfect or *nil*, just as in epileptic impulses or natural dreaming. I am satisfied these are important practical differences as to prognosis and treatment, and I conclude that my Irish patient was to be classed with the latter. I shall, however, refer to the question of the relations of memory to the delusions of the insane, and to illustrate this better, I subjoin a case:—

CASE III.—*Contused Wound on Occiput; Unconsciousness; after Recovery of Consciousness, Loss of Memory of Events, and Delusions of Place; Counter-irritation over Occiput and Forehead; Amendment.* (I am indebted to Mr M. Macleod, M.B., for notes of this case.)

George M'K., aged 53, of intemperate habits, was admitted into Ward X., Royal Infirmary, on 30th Nov. 1872, insensible, and with a contused wound in the occipital region, got in a fray. He came under the care of Dr Joseph Bell, who subsequently kindly asked me to see the case with him. On examination at his admission, it was observed that his pupils were equally contracted and immovable; the muscles of his left arm were rigid, and there was facial palsy on the left side, but he could shut the left eye. The following day at visit, he was able to tell his name, but was very slow in answering questions, and usually only said yes or no. He passed urine involuntarily. After a few days of suitable antiphlogistic treatment, it was thought advisable (on 5th Dec.) to let him have a better diet, to apply counter-irritants to the occiput, and to give him tincture of digitalis in combination with tincture of ginger.

For a while, subsequently to this, his intelligence improved rapidly, so that he answered questions more quickly; but his articulation was not distinct, the words running into each other, and he would lie with his eyes shut, but in a half-awake state, muttering to himself, and when roused, he had not the power of attention.

On 14th Dec. it was found that the pulse had improved, the facial palsy had almost disappeared, and the rigidity of the left arm had ceased, but he still passed his urine and fæces occasionally in bed. It was now observed, further, when he could speak better, that he had quite lost the memory of the most recent events, but was able to recall those which took place years ago, with some accuracy. He did not sleep well, for which he was ordered to have supper with a stimulant, and which was beneficial. On the 23d Dec. there was little improvement as to his memory, and he had now delusions as to the place in which he was, not recognising it or understanding it to be the Infirmary. We now thought it would be useful to blister the temples and give him a tonic—a combination of strychnine and iron. After this, the patient improved rapidly, and was able in a few days to remember recent events better; his delusions as to place ceased. He also laid with his eyes open and did not mutter to himself, and his bodily health also improved rapidly. A few days before his discharge on 10th Jan. 1873, his left pupil began to dilate, and so remained up to his leaving the Infirmary.

Although treatment was so beneficial in this case, the morbid state was far from its termination in health, when the patient left the Infirmary. A man aged 53, of intemperate habits, is little likely to escape from the sad fate of the last case. In both, local basilar symptoms were manifested, and in both, it is probable, the cerebellum and its sensory ganglia were shaken.

But in M'K. there were two points of difference: one at the beginning, as to the prolonged state of insensibility following upon the injury, and which was much more like sleep than coma—an important difference; the other that, when attention and perception became possible with returning functional activity of the cerebellum, he had such a condition, probably of the frontal lobes, as often occurs in senile dementia. In old age, the chief defect of memory is as to synesis and the recollection of recent events—in short, senile dementia is in the first stage senile *asynesia*, and with this there may be even a very vivid reminiscence of long past events—so vivid, indeed, that it is unnatural and is a hypermnesia. The conjunction of this with loss of memory of recent events, is a mystery to those who do not discriminate the two processes physiologically and pathologically. When, however, both are defective, and senile dementia is established, there is often also manifested characteristically the delusions as to persons and places which M'K. manifested. These also arise from a defect in two distinct processes. Firstly, there is no sufficient reminiscence; and, secondly, what reminiscences are excited by impressions from surrounding things, develop into delusions, which are not corrected, because of the defective reminiscence. If a man has a morbid recall, when his brain is otherwise healthy, he recognises the recall as erroneous and delusional; but when the impressions from external things do not induce normal reflex sensory action, then either hallucinations as to persons, places, and events will arise, or delusions from incoherent associations of ideas. This often occurs in old age, in insanity, and in delirium. There is no correcting recall, and so no discovery of error. This is also what occurs in dreaming. Impressions from external things being shut out in sleep, there is no knowledge of place or things, or of time past or to come, so that when there is reproduction, and excitation of association of ideas in an imperfect sleep, there is no correction by external impressions, no proper reminiscence and normal perception, and consequently there is no correction of the delusions which arise. Impressions as to time and place being shut out by closure of their inlets—the external senses—the dreamer has no proper conception of time and of his surroundings, and of the sequence of events in time or locality. Space is annihilated because not perceived, and moments are years.

It is a fortunate circumstance, that the exclusion of all external impressions during sleep prevents the process of synesis; if it were otherwise, our lives would be as incoherent as those of the delirious or the lunatic.

The pathological conditions under which motor synesis, in relation to reproduction and reminiscence, occur, are so numerous that I must defer their consideration.

ARTICLE II.—*Albuminuria in Cases of Vascular Bronchocele and Exophthalmos.* By J. WARBURTON BEGBIE, M.D.

(*Read before the Medico-Chirurgical Society of Edinburgh, 4th March.*)

THAT albuminuria may exist independently of Bright's disease, and be therefore unconnected with any structural change in the kidneys, is a proposition which few physicians, if any, will be prepared to call in question.

No doubt, the presence of albumen in the urine is the most significant feature in the symptomatology of Bright's disease, otherwise there could have existed no warrant whatever for the employment of the term as a synonym for that disease. The use of the expression in this way is however, faulty, and will become more and more unsuitable as the discovery of albumen in the urine, under other circumstances than those which indicate the existence of a grave renal disease in one or other of its different forms, is made. The presence of albumen in the urine as a notable feature in some cases of vascular bronchocele and exophthalmos has been familiar to me for some time; while a very special interest is attached to the circumstances under which the albuminuria has in a few instances been met with.

I have been unable to find any reference to the association of an albuminous condition of the urine with those remarkable symptoms, the union of which characterizes the disease now well known through the writings of Graves, Stokes, Basedow, Begbie, Trousseau, and other writers. This is, I believe, only to be accounted for by the circumstance that the condition of the urine has hitherto in this disease not been made the subject of any careful examination. It will of course be understood that I exclude from this reference the albuminuria of passive renal congestion, which is so often witnessed in cases of heart-disease, and likewise when the primary vascular impediment is seated in the lungs, and sometimes also in the liver. From this reference I also exclude those instances of vascular bronchocele and exophthalmos in their advanced stages, in which the heart, and, in their turn, not unfrequently the kidneys, have become involved in organic changes. In the latter, dropsy and albuminuria are constantly present, but under these circumstances they are to be properly regarded as symptoms, not of the disease itself, but of those complications which are prone to occur in its course.

The albuminuria incident to cases of vascular bronchocele and exophthalmos in their earlier stages is essentially a temporary albuminuria, and is evidently unconnected with any form of renal degeneration. But although temporary, disappearing as the other symptoms of the disease are either relieved or removed, the albuminuria has generally been considerable in degree, and sometimes even excessive.

In this respect the coagulability of the urine in cases of vascular bronchocele and exophthalmos has presented a remarkable contrast to the temporary albuminuria which is found in certain other diseases. I have indeed never seen so large an amount of albumen in the urine in any other disease, when the cause of the albuminuria was not inflammatory or organic.

My attention having been called to the association of an albuminous condition of the urine with vascular bronchocele and exophthalmos, in the first instance, owing to the existence, in certain cases, of œdema of the feet and ankles, I became satisfied that the association is by no means of infrequent occurrence. I have found albuminuria in a considerable number of the cases which have lately fallen under observation, and it is more than likely that it may have existed in other instances, although unrecognised from causes to which reference will be made. Albuminuria has existed in persons of both sexes suffering from this disease, and has been more common in cases of female than male patients; but when the much greater frequency of vascular bronchocele and exophthalmos among women than men is kept in view, the symptom has been present in a larger proportion of males than females. In some it has been an evanescent symptom, lasting only for a short time, and when so, only present in limited degree. In others, the albuminuria has been very considerable—it has even been excessive, and it has lasted for weeks, indeed for months—while the other notable symptoms of the complex malady continued, and only disappeared as the latter became relieved or removed. Œdema of the lower limbs, although in the first instance calling attention to the condition of the urine, has not been observed to bear any constant relation to the albuminuria; on the contrary, œdema, and sometimes considerable anasarca of the legs, have been present without any appearance of albumen in the urine; and albumen, when present, has generally existed without any form of dropsical swelling. In the most notable cases of albuminuria in connexion with vascular bronchocele and exophthalmos, dropsy has not been present.

In prosecuting my inquiry on this subject, a very interesting circumstance became manifest, namely, that the albuminuria was in certain cases limited to the period of digestion—present immediately after a meal, and absent when the person fasted. I had in one case been not a little puzzled by noticing the strange variety presented by the urine within very short periods—the albumen present in considerable quantity one day, and absent the next—present in the urine of the forenoon, and not to be detected in that passed before dinner. By obtaining repeatedly specimens of the urine in this case, and in one or two others, I was able to satisfy myself that in this disease the albuminuria is apt to possess the remarkable character of only occurring during or immediately after the digestion of the food.

This appears to me to be a most interesting feature, and it is also in various ways a very important one. The existence of albumen might readily escape detection if the physician relied on the results of one examination, as he is not unapt to do; and even when taking more than usual care, it is quite possible that in several specimens of urine furnished by the same patient for examination it might be found that no albumen existed. Conceive a patient so affected consulting a medical man before breakfast or luncheon; his urine, carefully examined, found to be non-albuminous; but, from some cause or other—and we know how fickle some patients are—calling for another physician shortly after a meal, when a large precipitate of albumen occurred on applying heat as well as on the addition of a little nitric acid. Remarkable as this character of the albuminuria which occurs in cases of vascular bronchocele and exophthalmos is, there is another feature pertaining to it which is even more striking. The albumen is present in much larger quantity after breakfast than after either luncheon or dinner. In one case of this kind, which I was able to observe at intervals for a considerable time, the urine presented the following characters:—It was passed in average amount, was of healthy colour, reaction, and density. On no occasion was the density observed to fall below 1015, and it never rose above 1025; the average density was that of health, 1020. Very occasionally this urine deposited lithates, and from time to time contained a slight excess of earthy phosphates. Sugar was never present. Albumen existed in this urine daily for upwards of a twelvemonth, but only at certain times of the day, and these times were readily found to be shortly after meals. After breakfast, however, the amount of the albumen was invariably greater than after luncheon, dinner, or an evening meal. Being greatly interested in these peculiar phenomena presented by the urine in the case of a gentleman whom I had recommended to spend the spring months in the south of England, I requested him to take an opportunity of consulting Dr George Johnson of London, whose opinion in all departments of urinary pathology is so deservedly held in high esteem. In connexion with this case, Dr Johnson wrote to me as follows:—"I saw two days ago a patient of yours, who asked me to write to you. As he told me that the explanation which I gave him of his symptoms is essentially the same as that which you gave him, I need not enter into minute detail in writing to you. I found that the urine passed after his breakfast contained a large amount of albumen; that passed at 3 P.M. contained none; that in the evening, three hours after dinner, contained a small quantity. He has been overworked, and is evidently a nervous, excitable man. I conclude that there is no structural change in the kidney, but that his kidney is irritable and congested at intervals during the process of excreting the products of faulty digestion. I confess that I am at a loss to account for the fact of the urine being more constantly and copiously albuminous after breakfast than after dinner. I repeated to him the usual directions as to avoiding cold and wet and fatigue, and long fasting. I also advised him to make trial of an exclusively milk diet. He told me that you had made the same suggestions to him. I prescribed a mixture of tincture of quinine and tincture of nux vomica, acid hydrocyanic diluted, and syrup of ginger, to be taken after food, and advised him to take an occasional dose of chloral hydrate when he is restless or disturbed by dreams.

"It occurs to me to ask whether the breakfast taken after a long interval from the previous meal, and when, consequently, the absorption of materials is likely to be more rapid, gets into his vessels quickly, and in a crude and half-digested state? If this be so, food taken at shorter intervals would seem to be indicated. The urine passed in my room an hour and a half after breakfast became nearly solid with heat and nitric acid. I have often met with cases in which the urine has been albuminous only after food and exercise, but I have not before met with one in which the *breakfast* appeared to be so especially noxious to the kidney."

A brief account of the case to which Dr Johnson alludes in his interesting communication just quoted, will now be given, before offering certain considerations which its observation has suggested.

A gentleman, of about thirty years of age, had long been in a somewhat delicate state of health, although no particular attention had been called to his symptoms till the summer of 1871. He was then suffering from debility, great nervousness, and palpitation of the heart. These had succeeded a condition of looseness of the bowels, which had existed for a considerable time. The prominence and peculiar expression of the eyes in this gentleman were, at the time now referred to, quite characteristic, and suggested the very probable discovery of a goître. This, although the patient had been quite unaware of its existence, was readily detected in the form of a soft pulsating tumour on both sides of the neck. The visible pulsation of arteries in the neck and limbs was present in a notable degree. On examining the urine of this gentleman, it was found to contain a considerable amount of albumen; but speedily the discovery was made that the albuminuria was not persistent, but, on the contrary, variable in its occurrence. The urine passed after meals was found to be more constantly and highly coagulable than that passed while fasting, and at times the urine was found to be free from albumen. Dr Henderson of Helensburgh, under whose care this gentleman had been, had noted the variable character of the albuminuria, and had distinctly traced its occurrence to the periods of digestion. Both before and after this gentleman's visit to the south of England and residence on its coast, rest from professional duties—which, in his case, were arduous—had been enjoined, and various experiments in diet and regimen had been practised. He had also several remedies in addition to those prescribed by Dr Johnson. From bromide of potassium and belladonna he undoubtedly received benefit, but more especially from digitalis. To the latter, indeed—which, in the form of its tincture, he took persistently for months, in doses of from ten to twenty drops thrice daily, frequently combined with iron, and sometimes alone—he has himself ascribed the chief benefit. Under its use the prominent eyes have retired, the goître has nearly if not entirely disappeared, the cardiac pulsations have fallen from 140 per minute to the normal standard, and the distressing palpitation from which he suffered has been succeeded by calmness in the heart's action. The nervousness and apprehension so characteristic of his malady, and which in his case were often most distressing, have given place to mental calmness and tranquillity. He has become greatly changed for the better in appearance; having been formerly very notably thin, he has now become very fairly nourished. He has returned to his professional duties, and during the past winter has discharged them regularly and comfortably. For months there has been no appearance of albumen in his urine. The last time I had an opportunity of examining the urine, which was passed shortly after breakfast, it contained no trace of albumen. The recovery in this gentleman has been complete.

It is surely a satisfactory consideration that a condition of excessive albuminuria—the urine becoming nearly solid on the application of heat and addition of nitric acid—may, after all, not indicate the existence of any structural change in the kidney. Of course, in connexion with the albuminuria, the presence or absence of certain other important features must, under such circumstances, be taken into account. Apart from the intermittent character of the coagulability, the facts that the quantity and density and colour of the secretion did not deviate from the healthy standard, and still more, that diligent and repeated examination by the microscope failed to detect the vestige of a cast of any kind, were to be regarded as the proofs of the renal derangement being functional and not organic. Still, there is occasion for reiterating the assurance that albuminuria is not Bright's disease, and for pointing out that, when unconnected with the presence of blood or pus in it, there may be even a highly coagulable condition of the urine, due to causes which are wholly independent of any structural change in the renal substance. Such, I am persuaded, may confidently be affirmed of the albuminuria which is apt to occur in cases of vascular bronchocele and exophthalmos.

Some little time after my attention had been called to the peculiar features of the albuminuria which is incident to cases of vascular bronchocele and exophthalmos, and to which reference has now been briefly made, a very instructive instance of the malady fell under my notice in the person of a medical man, who had been for some years engaged in active practice in the south of England. Calling one day for advice, the gentleman in question told me that he was the subject of Bright's disease, and feared that little or nothing could be done for his relief. He certainly looked ill, was

thin and sallow in appearance, and evidently deeply depressed in spirits. Having, however, noticed, as he entered my room for the first time, that he possessed the prominent eyes, with peculiar staring expression, so characteristic of vascular bronchocele and exophthalmos, I ventured—in reliance on my previous observations —to offer the comforting suggestion, that possibly, if not probably, the presence of albumen in his urine might be due to causes which were capable of being removed, and did not indicate the existence of any serious disease. This remark he received with politeness, but with very evident incredulity, mentioning that his condition had already been condemned by medical authority, that he did not expect to be cured, but only felt justified in expecting a little prolongation of life with greatly impaired health. On carefully examining this gentleman, a bronchocele of considerable size, soft, and pulsating, of whose existence he had been unaware, was discovered. His pulse was small, and as frequent as 140 per minute. The urine, on its earliest examination, while the patient was fasting, and between 1 and 2 P.M., was, to his own surprise, found to be free from albumen, its density 1020, and of acid reaction. Subsequent examinations of the urine determined its decided coagulability after meals, and its freedom from albumen while abstinence from food was practised. This gentleman was exceedingly nervous, and very desponding. Under treatment a considerable improvement took place. As he had occasion to pass through London, I begged him to see Dr Johnson, and it is to him Dr Johnson alludes in a letter of date 27th October 1872, from which I now quote:—"There is a striking resemblance between his case and that of the Scotch clergyman whom you were so good as to send to me. Dr —— is extremely feeble and nervous, and I fear that the prognosis is bad. I quite agree with you, that a long sea voyage would be the best course for him, but he seemed unwilling to do anything that would separate him from his family." I have not seen this gentleman for some time, but his progress can be traced in the correspondence I have had with him. On the 28th of March 1873 he wrote:—"I have gone very comfortably through the winter, considering all things. I am much stouter than when you saw me, and much stronger. I do a good deal of walking in the course of the day. My heart still beats very rapidly, but its action is not so irregular as formerly. Whether albuminuria exists, I cannot say, as I never test for it, and try to banish the thought of it altogether from my mind. The goître is decidedly less in size, but is still visible."

I obtained two specimens of urine passed on the 10th of April 1873. That voided before food was taken, contained deposit of lithates, and was absolutely free from albumen. That passed an hour after breakfast had a density of 1016, and contained a very faint trace of albumen. The letter which accompanied the specimens of urine will exhibit the peculiar nervousness under which my friend still laboured. "I send you," he wrote, "two little bottles with urine which I passed to-day. Please do not send me a bad report, as it will only frighten me. I dread the thought of renal disease so much, that I try to banish the very existence of kidneys from my thoughts. That I shall look with anxiety for your next letter, and yet dread its arrival, your knowledge of my nervous condition will assure you." Happily, I had no occasion to send "a bad report," while, in consideration of his highly sensitive and nervous state, I wrote over the seal of my communication, "good news."

Since the summer of 1873, this gentleman's condition has steadily improved. On the 28th October he wrote:—"I am glad to be able to tell you that my wife was confined on the 27th ultimo, and has done very well indeed. For myself, the only trouble I now have is occasional palpitation." Again, on the 8th December:—"My general health is now very good. I am as fat as ever I was, and my eyes have lost that unnatural stare. There is still a slight enlargement of the thyroid gland, and sometimes—though rarely—my kidneys act very little. Digitalis and iron remedy this. My heart still beats fast." My last communication, of date 17th Jan. 1874, gave a most satisfactory account of his progress, and was written a few days before he embarked as surgeon of a ship sailing with the royal mails for Madeira, Ascension, and the Cape of Good Hope. Before obtaining this appointment, my friend was obliged to go before the medical officer of the General Post-Office for examination as to personal and professional fitness. In this communication he says:—"I find no remedy relieve me so much as bromide of potassium, in 20-grain doses, thrice daily. Aconite and digitalis do not relieve me much."

This case is certainly a very gratifying and encouraging one. Not only has there been an almost entire disappearance of albumen from the urine, but the other symptoms from which the patient suffered in very notable degree—the palpitation of the heart and throbbing of the arteries, the bronchocele and exophthalmos—have all become very greatly lessened, and will, I fully expect, soon entirely disappear. This hope is justified by the results of experience in other cases. It has occurred to me to witness, within the past two years, three cases of vascular bronchocele and exophthalmos, all in females, in two of which albuminuria existed, gradually improve under the same treatment as that which has been pursued in the instances more fully detailed, and ultimately in these a complete cure has been obtained. In the second patient, whose case has been narrated at some length, renal casts were said to have been seen. This may very possibly have been so, but repeated careful examinations of the urine, made while he remained under my care, failed to detect their presence. I have, indeed, in no instance of the albuminuria occurring in cases of vascular bronchocele and exophthalmos, found renal casts. This remark applies equally to the examples of excessive albuminuria, and the more numerous instances of the disease in which the coagulability of the urine has neither been great nor long-continued. Albuminuria is not a constant symptom of vascular bronchocele and exophthalmos, but it is a frequent one. I am inclined to think that it may hereafter be found a more frequent symptom than my own observations presently entitle me to call it. From the circumstance that I have had no opportunity of carefully watching some instances of vascular bronchocele and exophthalmos which have recently fallen under my notice, having in such been able to make an examination of the urine only on a solitary occasion, it is very probable that albuminuria may have escaped detection. It existed in a patient of Dr Rosa, whom I saw lately, a married woman, æt. 36, the mother of seven children, in whom the malady succeeded the occurrence of lienteric diarrhœa and prolonged lactation. Dr Affleck informs me that he has found albuminuria, of the nature I have been describing, to exist in a sufferer from vascular bronchocele and exophthalmos recently under his care.

Having indicated the nature of the albuminuria which is found in certain cases of vascular bronchocele and exophthalmos, it now becomes necessary to inquire a little more fully into its pathology. Albumen is, in all probability, not a constituent of healthy urine. It has, indeed, been stated by Dr Gigon of Angoulême, that albumen exists in normal urine, and can be thrown down by chloroform. Becquerel, Aran, and Parkes have satisfied themselves that the precipitate which is produced by the addition of chloroform to the urine is not albumen, but a mixture of chloroform, mucus, and organic substances. Albumen, however, is so frequently present in the urine, and occurs under so vast a variety of circumstances, that it becomes a matter of very great importance to determine its clinical significance. The existence of blood, pus, or spermatic fluid in the urine renders it coagulable; but it is scarcely necessary to state that, in the cases of vascular bronchocele and exophthalmos to which I have referred, there were none of these conditions. Again, the excessive use of a diet composed chiefly or entirely of albuminous matter, such as eggs, has been found by various observers—among others, Barreswil, Hammond, and Brown-Séquard—to produce albumen in the urine. Barreswil, after taking ten eggs, passed albuminous urine for twenty-four hours. There can be little doubt that, in some persons peculiarly constituted, the partaking of certain articles of food difficult of digestion by them, produces albuminuria for a time. Of this nature was the case of the student mentioned by Sir Robert Christison, in whom a large amount of cheese or pastry produced albumen in the urine. Apart, however, from errors of diet, as Dr Parkes has stated, temporary albuminuria will occur in persons with very slight disease. Beneke, when suffering from dyspepsia, noticed albumen in his own urine four times in four weeks. Clemens, Rayer, Martin Solon, and many other physicians, have made similar observations. It may be admitted, then, that albumen, although not an ingredient of healthy urine, may occur in the urine of healthy persons, or of persons whose disorder of health is, at the time of its presence, very slight. Attention has recently been called by Dr George Johnson to the occurrence of albuminuria in healthy persons after bathing in cold water. Again, albuminuria is apt to occur in relation to a great many disorders which are not essentially connected with structural change in the kidneys. Not to dwell upon pregnancy, as a condition of the system with which albuminuria is associated, there is the puerperal state. Again, there is a large number of febrile and inflammatory diseases in the urine of which albumen very often occurs. Among these may be mentioned scarlet fever, measles, erysipelas, smallpox, diphtheria, typhus and typhoid fever, cholera, etc., and, of inflammatory diseases, pneumonia. Over and above these relationships, there are various forms of visceral disease—of disease affecting the heart, liver, and lungs—in the urine of which albumen appears; and the physician, in his observation of such cases, is on the outlook for its occurrence, and ascribes it, when it does come, to the general impediment to the circulation which the following diseases—(I name them as illustrative examples only) dilatation of the heart, cirrhosis of the liver, emphysema of the lungs—produce; for, owing to these, the renal circulation necessarily suffers. In such diseases, the albuminuria is almost invariably associated with a diminished secretion of urine. It is not necessarily so in the albuminuria of vascular bronchocele and exophthalmos,—the quantity is generally unaffected, and so are the other characters of the urine.

Dr Roberts of Manchester has very clearly pointed out, that, in endeavouring to determine whether the presence of albumen in the urine be dependent upon the existence of organic disease of the kidneys or not, the question in each individual case must be considered in connexion with the three following points:—1. The temporary or persistent duration of the albuminuria; 2. The quantity of the albumen present, and the occurrence and character of a deposit of renal derivatives; 3. The presence or absence of any disease outside the kidneys which will account for the albuminuria.[1]

Now, viewing the albuminuria of vascular bronchocele and exophthalmos under these aspects, the following observations may be made:—1. The albuminuria is temporary; for, according to Dr Parkes's definition of that condition, it has totally disappeared while the patient is under observation; but, instead of lasting a few days or weeks, as holds true of most instances of temporary albuminuria, properly so called, it has lasted for many months, indeed for a year. Intermittent or remittent albuminuria would be a better signification than temporary, for the albuminuria of vascular bronchocele and exophthalmos, but, better still, because more definite, albuminuria occurring during or after digestion. 2*d*, The amount of albumen which is present in the urine when the cause of its manifestation is independent of organic or inflammatory disease is usually small; very often it is not more than a mere trace. The amount of albumen in the urine when passive congestion of the kidneys results from cardiac or other visceral disease may indeed be considerable, but I do not remember ever to have seen the urine under such circumstances very highly coagulable; very highly or excessively albuminous has, however, been the character of the urine in at least one example of vascular bronchocele and exophthalmos, the recovery in which has been complete. Again, in the temporary albuminuria of other maladies, there is generally some deviation, often notable, from the normal condition of the urine in other respects; the quantity, density, and reaction of the urine are often affected; and there is the presence of lithates in excess, or an undue amount of earthy phosphates. Not so, generally at least, in the cases of vascular bronchocele and exophthalmos which I have seen. With the exception of its containing albumen in considerable or large amount, the urine has been healthy. Neither has there been in the latter any deposit derived from the kidneys. In this respect, indeed, there is the interesting fact of a copious presence of albumen in the urine, without any trace of casts of one kind or another, and without any renal epithelium or blood. Further, the form of albuminuria which we are now considering, differs from any other form hitherto described, in being limited to the period of digestion of the food. *3d*, The presence of disease apart from the kidneys is of course conspicuous in vascular bronchocele and exophthalmos, while the peculiar morbid condition of the nervous system and of the bloodvessels in that disease, as well as the spanæmia which exists, must, I think, be taken into consideration in our endeavour to determine the pathology of the albuminuria, which we now know to be in some way or other associated with it.

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[1] On Urinary and Renal Diseases, 2d edition, page 172.

In vascular bronchocele and exophthalmos, there is always present much disturbance of the nervous system—the sufferers from this disease are invariably highly nervous—they are often hysterical. The primary disorder of the circulation, both cardiac and vascular, is of the nature which we associate with derangement of the nervous system. The organs and parts of the body in which the local manifestations of disturbance are seated, are organs and parts freely supplied with bloodvessels and blood,—the thyroid gland, the spleen, which, although not invariably, is often affected, and the deep ocular tissues. To these must be added the kidney. From the failure of due nervous influence, the small vessels, and, it may be presumed, the capillaries, in the thyroid gland, and the deep-seated orbital vessels, become dilated, and the circulation through them in consequence interfered with. We can infer from the consideration of the essential nature of the renal circulation that if an obstruction to the return of blood through the inter-tubular capillaries and veins exists, either from an obstruction in the heart or lungs, or from a disordered state of the vessels themselves—a condition which I believe to exist in vascular bronchocele and exophthalmos—favoured by the more or less watery state of the blood itself, there will occur a transudation of serum, carrying with it albumen, through the walls of the Malpighian capillaries into the tubes, and thus the urine will be rendered coagulable.

But in order to explain the limitation of the albuminuria to the period during and after digestion of the food, it is necessary to regard the increased afflux of blood which then takes place, as leading to an altered physical relation between the blood and the walls of the vessels, and likewise determining an engorgement of the Malpighian capillaries, while the loss of tonicity in the efferent vessels is thus rendered temporarily more injurious. In other words, the renal circulation, in its comparatively tranquil condition, is unaffected by the disordered state of the capillaries and small vessels; but, when excited by the stimulus of a recent meal, it is unequal to the task, and the resulting interference determines the albuminuria.

But, further, there is something in the character of this albuminuria to ally it with the albuminuria of indigestion, to the occurrence of which a brief reference has been made. Sufferers from vascular bronchocele and exophthalmos have frequently an inordinate appetite and craving for food. They have bulimia; and, in the case I have shortly recorded, this symptom was notably present. A large meal taken hurriedly is not unlikely to influence, as a remote cause, the production of the albuminuria. I think Dr Johnson's explanation of the greater amount of albumen in the urine after breakfast than dinner, may be correct—the same explanation had occurred to my own mind—that the earlier meal taken after a long fast gets into the bloodvessels quickly, and in consequence leads to a greater disturbance of the renal circulation. Besides, hot tea and coffee, with eggs, consumed at breakfast, may be presumed to be articles more likely than others to furnish to the blood the offending material.

I have not attempted to exhaust this interesting subject, but have for the present limited myself to pointing out the occurrence of a form of albuminuria which, so far as I am aware, has not in its details been previously described by any observer, namely, albuminuria occurring during and after digestion in cases of vascular bronchocele and exophthalmos.

ARTICLE III.—*On the Size of Aperture necessary for the Passage of the Placenta and for the Passage of the Accoucheur's Hand.* By J. MATTHEWS DUNCAN, President of the Obstetrical Society.

(Read before the Obstetrical Society of Edinburgh, 25th February.)

AT a late meeting of the Society I raised the question of the dimensions of an aperture such as the os uteri, when just capable of transmitting the accoucheur's hand, and its dimensions when just or earliest capable of transmitting the placenta. These questions had evident bearings upon the methods of treatment of placenta prævia, which was the subject under discussion; and, as their solution was not known to myself or to the members present, I resolved to attempt to settle them.

The first of these two questions which I shall consider is the smallest aperture capable of transmitting a placenta uninjured. It is evident that the aperture will be circular, or nearly so, and in the experiments I used a circular aperture. It is also evident that, with a view to exactness in the experiments, and with a view also to the practical utility of them, the placenta should be brought through uninjured. Indeed, injury or laceration would make little difference in the results, unless the organ were greatly torn; for, supposing the organ to be circular and of equal thickness in all its parts, laceration would not facilitate transmission through a small aperture, unless the laceration was such as to remove from the aperture a part of placenta which would otherwise be there while the thickest part or centre of the placenta was passing.

Mr A. C. Harris, one of the resident physicians of the Royal Maternity Hospital, kindly undertook to perform for me the following experiments, and it is the results which be obtained that I now give.

TABLE *of Experiments showing the size of Aperture through which the Placenta can be Extracted with more or less difficulty.*

No. of Experiment.	Weight of Placenta.	Diameter of Placenta.	Thickness of Placenta.	Diameter in Inches of Aperture of Transmission.					
				1¼	1½	1¾	2	2¼	2½
1.	1 lb. 1 oz.	7 inches.	¾ inch.	...	*e*	*d*	*a*	...	...
2.	1 „ 5 „	6½ „	1 „	...	...	...	*d*	*d*	*a*
3.	1 „ 4 „	7 „	¾ „	...	*e*	*c*	*b*	...	...
4.	1 „ 1¼ „	6¼ „	¾ „	...	*e*	*c*	*a*	...	...
5.	1 „ 2½ „	6½ „	1 „	...	...	*c*	*b*	*b*	*a*
6.	1 „ 2 „	6½ „	½ „	...	...	*c*	*b*	*b*	*a*
7.	1 „ 5 „	8 „	½ „	...	...	*d*	*b*	*b*	*a*
8.	1 „ 6 „	6¼ „	1½ „	...	...	...	*d*	*b*	*a*
9.	1 „ 4 „	7 „	1 „	...	...	*d*	*c*	*b*	*a*
10.	1 „ 4 „	6½ „	1 „	...	...	...	*d*	*c*	*a*
11.	1 „ 6¼ „	7 „	1 „	...	...	*d*	*c*	*b*	*a*
12.	1 „ 1½ „	7 „	¾ „	...	...	*c*	*b*	*b*	*a*
13. & } Twins.	1 „ 6 „	7 „	½ „	...	...	*c*	*b*	*b*	*a*
14.	14 „	6 „	¼ „	*e*	*c*	*b*	*a*	...	...

In this Table—*a* = Quite easily. Its own weight sufficient to pass it.
b = Easily. Placenta requiring careful folding.
c = With slight difficulty. Requiring folding and slight pushing.
d = Difficultly. Requiring folding, pushing, and pulling.
e = Very difficultly. Requiring, in addition, time and nice manipulation.

The Table shows that an aperture of fully two inches in diameter is required for the transmission of the uninjured mature placenta.

Of course, in a much lacerated state, it may be brought through an aperture of smaller dimensions.

I do not remember any statement of such results as I have just given with reference to the placenta, nor do I know of any similar statement regarding the transmission of the hand. But this latter point has been described, for practical purposes, in a valuable practical way, by many authors, and of this kind of description I shall give examples. It is evident that all of them are purely practical statements, and do not in the least solve the important question which is raised in this short paper.

Burns (Principles of Midwifery, etc., 10th edition, p. 370) says—"We may safely deliver whenever the hand can be introduced without much force." Collins (Practical Treatise on Midwifery, p. 94) says—"The earliest moment that the mouth of the womb will, by gentle efforts, permit the introduction of the hand." Rigby (System of Midwifery, p. 262) says—"When once the os uteri is sufficiently dilated to admit the hand." Ramsbotham (Obstetric Medicine and Surgery, 5th edition, p. 369) says—"When it (the os uteri) has acquired the diameter of half-a-crown (1¼ in. diam.), or a crown (1½ in. diam.), it will generally suffer itself to be dilated to such an extent as will admit the hand without injury to its structure . . . . (p. 370). As soon as the mouth of the womb is sufficiently open to admit the thumb and the four fingers as far as their second joint, we may expect that it will offer but a slight impediment to the passage of the whole hand." Murphy (Lectures on the Principles and Practice of Midwifery, 2d edit., p. 457) says—"If the os uteri be one-half, or even one-third, dilated, you may deliver."

All of these statements, and I might quote more of them, merely imply that you should pass your hand if you can, or if you dare. The terms of caution have no definite meaning, and are of little value, being interpreted in a different way by different minds. Now, what I desire to make out is the answer to the question—How large must the os uteri be to transmit the accoucheur's hand? or, to what extent must it be expanded for this purpose? The authors just quoted deal with and attempt to describe that stage of dilatation at which farther dilatation to the necessary amount for passing the hand may be expected. Their determinations are almost worthless, as already said; for it is very difficult to estimate the size of the os uteri, and more depends on its softness and dilatability than on its size. I have delivered, passing the hand and turning, when the os at the commencement of the operation had undergone no active dilatation. The operation was easy, and without any injury to the cervix that I could ascertain, it being soft and dilatable.

The os, supposed to be circular, must have a diameter of from 2½ to 3 inches in order to transmit the hand of the accoucheur. My own hand can be forced through a round aperture of 2¼ inches in diameter.

In conclusion, I offer a single remark, having a practical bearing, that almost naturally follows the above measurements.

The placenta can be got away without disruption through an os of 2 inches in diameter or a little more, and the hand can be passed through an os of 2½ inches in diameter or a little more. The placenta is soft and lacerable, and can exert little force in distending the os. The hand is hard and firm, and can be used to dilate with considerable force. The os uteri is, in cases of placenta prævia, very rarely rigid and undilatable. It may, therefore, be safely asserted that, in the very great majority of cases of placenta prævia, the hand may be passed into the uterus, if the placenta can be extracted from it in a satisfactory manner without disruption.

This is not the place to enter on evident farther applications of this result, in unfavourably criticising the treatment of placenta prævia by separation and removal of the organ.

ARTICLE IV.—*Aspiration as a means of treating Cases of Urgent Retention of Urine.* By DR JOSEPH BELL.

(*Read before the Medico-Chirurgical Society of Edinburgh, 4th February.*)

ON the first appearance of the aspirator in surgical practice, I felt that by it we had a means of easily and safely treating certain cases of retention of urine; and when I showed it to this Society nearly two years ago, I alluded to such possible uses, in addition to the more evident and familiar advantages it possesses, as a means of diagnosing and treating abscesses and collections of fluid. The literature of the various uses of this instrument is already very extensive. I have notes of cases in English, American, German, French, and even Russian surgery, in which it has been used to diagnose and empty abscesses, cysts, cases of spina bifida, encephalocele, hepatic abscess, hydatid cyst, hernial tumours, urgent distention from flatus, etc. A few cases of its use in retention of urine have also been recorded, but very few; and the one I am now going to read is, so far as I can discover, the first in which this instrument has been used for this purpose in Scotland, and one of the very earliest in Britain; besides, it in itself was a case of great urgency and difficulty, and consequently of some interest.

A. R., æt. 65, was admitted under my care in the Royal Infirmary, on 18th June 1873, suffering from retention of urine and a variety of symptoms connected with it.

State on admission.—Patient a feeble, puffy old man; was in a semi-comatose state, but could be roused and could answer questions. His eyelids were swollen and his limbs œdematous. He had a weak rapid pulse and a harassing cough, along with the physical signs of bronchitis and emphysema. His scrotum and penis were enormously swollen, and of a dark red colour. There were several sinuses in the scrotum and perineum. The perineum was very thick and brawny. He had passed no urine by the urethra for three days, but a few drops had dribbled away by the sinuses in the perineum. His bladder could be felt distended nearly up to the umbilicus. The patient has a reducible scrotal hernia on the right side, which renders the urethra tortuous.

Previous history.—He had had a stricture for many years. Mr Syme cut him fourteen years ago, with great temporary improvement. I, when in charge of Mr Syme's ward, had dilated his stricture to full size about five years ago; but since then, until six months before admission, he had no treatment at all, and his stricture had become much worse. For six months he had been in an hospital under treatment by a surgeon, who had tried very frequently, but without success, to get any instrument, even the smallest, into his bladder.

I at once tried to pass small instruments, and found that a stricture existed about two inches from the orifice, which yielded readily to dilatation; but behind this, there was a track of tight urethra coated with phosphatic salts, and densely hard. In the condition the patient was, I did not deem it prudent to perform perineal section, so inserted No. 2 needle of Dieulafoy's aspirator in the middle line, about 1½ inches above symphysis pubis, and by it drew off with great ease about ℥xx. of fœtid turbid urine, of specific gravity 1017. This yielded albumen to a very great extent, about one-fourth of its volume. The needle was at once withdrawn, and the patient was placed in a warm bath. Great relief followed. In a few hours the patient began to be able to get some urine passed. On the third day I passed a No. 2 elastic bougie into the bladder, and in a short time dilated the urethra to admit No. 11. Several small phosphatic concretions have been passed, and the patient has regained strength. No trace of the puncture was visible after the first few days, nor did it occasion the slightest inconvenience. In fact, the patient voluntarily remarked that it was the easiest way of having the water drawn off he had ever experienced.

The patient made a good recovery and left hospital with a urethra admitting No. 12. The stricture will doubtless return unless he takes care of himself, and submits himself to frequent surgical treatment.

Cases admitting or requiring this treatment will not likely be very frequent—indeed, I have not met with another out of a very large number of stricture cases seen since June; still, in this one, any other treatment would have been very dangerous.

Perineal section is always tedious, requiring chloroform, which the weak heart and emphysematous lungs and diseased kidneys would have borne ill; besides, perineal section has its own dangers in old exhausted subjects. Tapping by rectum would have been difficult from the enlarged prostate. Catheterization had failed. The operation was painless, and left no trace. I have a very strong feeling that in similar cases, the aspirator gives us an easy, safe, and

reliable means of tiding over a difficulty, emptying the bladder, and thus giving time for other treatment.

It is possible, if necessary, to repeat the aspiration frequently in the same region, but not exactly the same situation.

The special merit of the aspirator here is, that it enables us, by the suction power it possesses, to withdraw the urine through a tube little larger than an acupressure needle, the wound inflicted by which heals up at once and leaves no trace.

ARTICLE V.—*Valedictory Address to the Medico-Chirurgical Society of Edinburgh, delivered on the occasion of retiring from the Chair of the Society, 7th January* 1874; by P. D. HANDYSIDE, M.D., F.R.S.E., formerly Senior President of the Royal Medical Society, and Senior Acting Surgeon to the Royal Infirmary; Teacher of Anatomy in the Edinburgh School of Medicine.

(*Continued from page* 787.)

VI. PATHOLOGY.

PATHOLOGY, or our knowledge of the Facts of Disease, is that ring of the series already referred to, touching at its inner circumference Physiology, and within which are disposed in successive rings, Anatomy, Zoology, Chemistry, and Animal Physics; while, at the outer circumference of Pathology are, in succession, concentric circles of Materia Medica, Therapeutics, Surgery, Medicine, Obstetrics, and Medical Jurisprudence.

Pathology may be regarded as the indelible distinction of our Society. Discountenancing the false idea that we have more to do with the uses of things than with their nature, Abercrombie —one of our founders—impressed by his example, followed to this day, the supreme value and importance of exhibiting Morbid Specimens at our meetings, as the only basis of *scientific* practice; Pathological investigations having in many instances opened up an altogether new meaning for many old words, such, for instance, as "Concussion of the Brain," "Concussion of the Cord," etc. In a "Circular," dated 20th November 1830, Dr Abercrombie, then President of this Society, submitted to the individual members a "plan for promoting its usefulness;" a sentence or two of which may be quoted. He says:—"Every member must often meet with facts of the greatest importance, and from which the most valuable deductions might be procured, provided an extensive collection of such facts could be viewed in connexion with each other. One of the causes, indeed, which retard the progress of medical science, appears to be the want of opportunities for such extensive comparison of individual facts; and from this source, probably, have arisen many of those partial deductions which have from time to time been proposed with much appearance of truth, and soon found to be fallacious. . . . No lengthened or studied communications are required, but single cases, simply and shortly detailed,—or even single facts, derived from cases which in other respects may present no particular interest. Such observations will, in the first instance, be valuable by giving rise to conversation, in the course of which additional facts may be communicated." Dr Abercrombie goes on to propose, "that select committees shall classify and generalize these facts, with observations pointing out the general conclusions which arise out of the whole treasure of pathological and practical truth."

The first paper read in this Society was one, on 5th December 1821, by *Dr Abercrombie*, entitled "Contributions to the Pathology of the Heart,"—*Part First.* At the same meeting, Papers on the following Pathological subjects were "presented to the Society:"—"Remarks on the Exfoliated Cranium of a Man who had died of Syphilis—the Skull exhibited," by *Dr* (afterwards *Sir*) *George Ballingall;* "Description of a particular Disease, to which the Bones of the Cranium are liable," by *Mr James Russell;* "On the Appearances observed on the Dissection of two out of three individuals found dead in the vicinity of Leith, supposed to have perished during the storm of 3d November, with some Reflections on the Pathology of the Brain, by *George Kellie, M.D.;*" "Some Observations on the Healthy and Diseased Structure of the Bones," by *Mr John Lizars,* Surgeon. At a subsequent meeting—16th January 1822—*Dr Abercrombie* exhibited to the Society "A Preparation of Fungous Tumours of the Bladder, which, by pressure on the Sigmoid Flexure of the Colon, produced symptoms of Enteritis;" also, "A Diseased Common Iliac Artery causing Gangrene." In 1822, papers were also received from this singularly endowed and eminent physician, entitled, "Contributions to the Pathology of the Stomach and neighbouring Viscera," Parts I. and II.; from *Dr Kellie,* the second part of his paper on the "Pathology of the Brain;" from *Dr Andrew Duncan, jun.,* "Dissection of an unusual Case of Congenital Hydrocephalus;" from *Dr W. Moncrieff,* "Case of Tubercular Disease of Peritoneum and Omentum;" and from *Dr Duncan, jun.,* "On Diffused Inflammation of the Cellular Membrane." *Dr Abercrombie,* at one meeting of the Society, exhibited a Stomach, in which a perforation had been found on dissection, and mentioned the particulars of the case. In 1823, Parts III. and IV. of his "Contributions to the Pathology of the Abdominal Viscera" were read, and also the second and third parts of *Dr Duncan's* paper on the Cellular Membrane; whilst among other valuable papers were those of *Drs Cullen* and *Carswell,* "On Melanosis;" "Description of a very dangerous disease of Infants, in which Erosion and Perforation of Stomach are found after Death," by *Dr John Gairdner;* "Case of Phlebitis of the Cephalic Vein, with the Appearances on Dissection," by *Dr Duncan, jun.;* "On Dysphagia, with an Abscess involving Œsophagus, Trachea, and Lungs," by *Dr David Hay*; "Notice of some Dissections which appear to illustrate the formation of Tubercles, by *Dr W. P. Alison*; "Remarks on the Nature and Origin of Tubercular Diseases," by *Dr Abercrombie*; and, "Illustration of the Pathology of Tubercles," by *Dr Kellie*.

In 1824, we have "Notice of some Remarkable Cases, with Dissections," by *Dr Alison*; and by the same author, "Notice of some Additional Cases, with Dissections." "Three Cases of Rupture of Vomicæ into the Cavity of the Pleura, accompanied with Tintement Metallique, and Pleurisy, and Pneumo-Thorax," by *Dr William Cullen*. "Case of Mollities Ossium, seemingly dependent on a peculiarly diseased Condition of the Medullary Membranes and Cavities, with Illustrative Drawings," by *Mr John Howship, London*. In 1825, "Memoir on Intestinal Herniæ, without a Hernial Sac," by *M. le Doct. Al. Colson, Paris*. "Foreign Body found in the Cavity of the Heart," by *Dr J. S. Combe*, "and on the question, Whether it was Organic or a mere Clot? *Drs Kellie and John Thomson* took opposite sides." In 1826, "On Congenital Disease of the Thigh-bone," by *Dr Knox*. "Case of Abscess of the Liver bursting into the Stomach," by *Mr Alex. Watson*. "On Liability of Animals, brought from Warm Climates, to be affected with General Tubercular Disease, with Dissection of a Paca from Brazil," by *Dr R. E. Grant*. In 1827, "On the sudden, spontaneous Obstruction of the Trunks of the Larger Arteries," Parts I. and II., by *Mr Turner*. "Additional Cases and Observations, illustrating the Origin of Tubercles," by *Dr Alison*. "A Specimen of Extensive Separation of the Internal from the Middle Coat of the Common Iliac, with complete Obstruction to the Circulation," was shown and described by *Dr Abercrombie*. *Dr Cullen* read a "Case, with Dissection, of Circumscribed Gangrene of the Lungs," and exhibited a Drawing of the parts. In 1828, "Case of a Congenital Disease or Malformation of the Thigh-bone, illustrating the Pathology of the Interstitial Absorption of the Thigh-bone," by *Dr Knox*; "Case of Sudden Death from Rupture of the Right Ventricle of the Heart, with Dissection," by *Dr Dewar of Stirling*. "On Painful Sub-cutaneous Tubercles, with Cases and Histories (with Engravings)," Parts I. and II., by *Mr William Wood*. "On Neuroma, with Cases and Histories (with Engravings)," by *the same*. "On Molluscum covering the whole Body,—shown to the Society," by *Mr Alex. Watson*. "On a remarkable Alteration in the Structure of the Patella, produced apparently by the presence of a False Cartilage," by *Dr Knox*. In 1829, "Case of Cerebral Effusion, with Perforation of the Stomach; and two other Cases," by *Dr Hood of Kilmarnock*. In 1830, "A peculiar Ulceration of the Aorta was exhibited, and Account of the Case given," by *Dr J. C. Gregory*. "Pathological Changes in the Eye, illustrated by Preparations," by *Mr Watson*. "Aneurism of the Abdominal Aorta, with Preparation," by *Dr Christison*. In 1831, "On Tubercular Disease of the Cerebellum," by *Dr Abercrom-*

bie. "Hydrocephalus connected with Disease of Cerebellum," by *Dr Gregory.* "Two Cases of Fungous Tumour of the Cardia," by *Mr W. Brown.* "Perforating Ulcer of the Duodenum," by *Dr Abercrombie.* "Hypertrophy of the Brain with Spontaneous Obstruction of the Humeral Artery, and other Morbid Changes," by *Dr Christison.* "Dissections of three Cases of Indian Cholera, with Remarks on the Treatment," by *Dr J. G. Stuart, H.E.I.C.S., Bombay.* In 1832, "Paper on Cholera," by *Professor Delpêch* and *Dr Coste of Montpellier, Dr Leowenhayn of Moscow,* and *Professor Lizars of Edinburgh.* "Several Cases of Cholera, with Examinations after Death," by *Dr John Mackintosh.* "An Account of some Additional Examinations after Death, in Cases of Cholera," by *Dr Gregory.* In 1833, *Dr Van Den Busch of Bremen* communicated a "Case of Rupture of the Stomach in a New-born Child." "Observations on the Formation of Hydatids," by *Mr John Howship of London.* "Several Preparations and Drawings of Fungus Hæmatodes of the Eye," by *Dr J. A. Robertson.* In 1834, "Case of Obliteration of the Vena Cava Descendens," by *Dr John Smith* and *Dr John Reid.* "Case of Spontaneous Luxation of the Vertebra Dentata," and "Cases of Obstruction of the Vena Cava Superior," by *Dr William Thomson.* "Observations on Phlebolites," by *Dr John Reid.* In 1833-4, *Dr Alison* read, "Cases illustrating certain Morbid Changes which the Blood appears to undergo in different Diseases." In 1834, *Mr Brown* read, "On some diseased States which resemble Inflammation;" *Dr Ransford,* "Case of Disease of the Bladder following an Injury;" *Dr J. Scott,* "On Perforations of the Intestines, with external opening;" *Dr J. H. Balfour,* "Case of peculiar Disease of the Skull and Dura Mater." In 1835, a paper was read "On the occurrence of Black Expectoration and of Deposition of Black Matter in the Lungs of Coal-miners," etc., by *Drs Dewar* of Dunfermline, *Mr Philp* of Aberdeen, and *Mr Steele* of Craighall, etc. etc. *Mr Watson* read a "Case of Globular Polypi or Excrescences within the Heart;" *Dr W. Thomson* read, "On Melanosis and Black Infiltration of the Lungs;" *Dr John Reid,* "Case in which Fibro-Cartilaginous Bodies were found loose in the Cavity of the Abdomen, with notice of similar cases;" *Dr Alison,* "Case of fatal Jaundice in which no Bile was found in the Gall-ducts;" *Dr J. A. Robertson,* "Case of Congenital Hydrocephalus, with Hernia Cerebri;" *Dr W. Thomson,* "Case of Sudden and Unexpected Death occurring in an individual whose Heart had only one Coronary Artery, the Coats of which were studded with Patches of Cartilaginous and Ossific Depositions;" *Dr Craigie,* "Three Cases of Ulceration with Perforation of the Stomach." In 1836, *Dr J. Y. Simpson* gave two communications "On Diseases of the Placenta;" *Dr Knox,* "On the Organic Changes in the Lungs, supposed to be produced by Pulmonary Apoplexy;" *Dr W. Thomson,* "Abstract of Cases in which Pseudo-Membranous Substances have been discharged from the Bowels." In 1838, *Dr J. Y. Simpson* read, "Notices of Cases of Peritonitis in the Fœtus in Utero." In 1839, *Dr John Reid*, "On some Lesions produced in the Lungs by Slow Respiration;" *Dr Robert Paterson*, "On Softening of Textures and Encephalic Serous Effusion taking place subsequently to Death;" *Dr John Smith*, "Cases illustrative of the Pathology of Insanity;" *Dr J. Y. Simpson*, "Case of Discharge of a Portion of Intestine by Stool;" *Mr W. Brown*, "On some States which resemble Inflammation;" *Dr T. T. Crawford*, of Tynemouth, "On the Black Infiltration of the Lungs occurring among Persons not exposed to the Inhalation of Carbonaceous Particles." In 1840, *Dr Peebles* read two communications "On Morbid Appearances in Cases of Great Enlargement of the Stomach;" and *Dr W. Young*, a "Case of Aortal Aneurism which opened into the Vena Cava Inferior." In 1841, *Dr J. R. Russell* gave a "Summary of Professor Rokitanski's new Investigations and Views relative to the Morbid Anatomy of Continued Fever;" *Dr J. A. Robertson* communicated the details and exhibited preparations illustrative of a series of "Cases of Injuries of the Head;" *Dr W. Henderson* "exhibited and explained the Structure of the Masses of Molluscum Contagiosum;" *Mr Spence* "exhibited a Specimen of Ulceration of the Stomach, in which Death from Hæmorrhage ensued from the Erosion of a large Blood-vessel." In 1842, *Mr John Goodsir* read, "Observations on the Morbid Anatomy of Continued Fever;" *Dr Henderson* "exhibited Preparations illustrative of the Pathology of Aneurism;" *Mr John Goodsir* gave "The result of his Researches into the Nature of the Degeneration which the Kidney undergoes in Bright's Disease;" *Dr Henderson* showed a case of Malignant Disease of the Lung; *Mr John Goodsir* gave "A Microscopic Demonstration, and Drawings of an undescribed form of Infusorial Vegetable—the Sarcina Ventriculi—ejected from the Stomach in a case of Dyspepsia;" *Dr Bennett* read a paper "On the Vegetable Nature of Tinea Favosa, including its Pathology," etc.; he read another "On Abnormal Nutrition (commonly called Inflammation), and on the mode in which its various Products are developed." In 1843, *Dr Bennett* read a paper "On the Nature and Varieties of Cerebral Softening;" *Dr James Duncan* gave a "Case of Spontaneous Obliteration of the Aorta;" *Drs Graham* and *Allen Thomson* mentioned each a similar case; *Dr P. W. Maclagan*, Army Surgeon in Canada, communicated a "Case of Disease of the Ileum, with Post-mortem six hours after death;" and *Dr P. D. Handyside* exhibited "Preparations, Casts, and a Drawing of a Case of Osteo-Medullary Sarcoma of the Thigh-bone, in which he had amputated successfully at the Hip-joint." In 1844, *Mr T. M. Lee* gave a "Case of Hydatids in the Cavity of the Abdomen;" *Mr Goodsir* gave a "Report of the Lesions discovered in the Post-mortem Examination of the Body of the late Dr Abercrombie." In 1845, *Dr Bennett* read "The Anatomical Evidences of the frequent spontaneous cure of Phthisis Pulmonalis, and the indications furnished by Pathology for its rational treatment;" *Dr Maclagan's* "Case of Sudden Death from Laceration of the Substance of the Heart;" *Dr Andrew's* "Case of Sudden Death produced by the rupture of a portion of the Muscular Fibres of the Heart;" *Dr Bennett's* "Case of Suppuration of the Blood, independent of Inflammation;" *Mr Goodsir's* paper "On a Tumour of the Testicle, containing an Osseous Mass, and covered with Integumentary Membrane and Hairs;" *Dr Bennett* exhibited Thibert's Models of Pathological Anatomy;" *Dr P. D. Handyside* exhibited the "Structure of an enormous Fibrous Cyst which had impeded Gestation," in a case under Dr Sidey's care. In 1846, *Dr Bennett* exhibited "Fragments of Exudative Masses in Laminæ which had been passed *per anum*, containing Cryptogamic Vegetations;" *he* exhibited, also, "a Pocket Compound Microscope, by Dr Gruby of Paris, for Pathological Purposes." In 1847, *Dr George Paterson* gave the Pathological Appearances in a "Case of Colloid Cancer of the Peritoneum;" *Dr Bennett* followed with a paper,—"Observations on Cancer in general;" *Dr Lee* read "Two fatal Cases of Tetanus, with Dissections;" *Dr Bennett*, "On the Morbid Anatomy and Pathology of Abdominal Typhus." In 1848, *Mr James Miller* read an "Account and Dissection of a case of Sacculated or Encysted Calculus removed by Lithotomy;" *Dr W. T. Gairdner* read a paper "On the Pathological Nature of Bright's Disease;" *Dr Alexander Wood* read "Remarks on Fistulous Communications between various Viscera." In 1849, *Dr Begbie* read his paper "On Anæmia and Goître," with Pathological References; *Dr Wm. Robertson*, "Observations on the Blood of Cholera Patients;" *Mr Syme* "exhibited a Preparation and Cast of a Tumour from the Cheek, which Mr Goodsir and Mr Cobbold described as consisting of a Convoluted Plexus of Varicose Veins, containing Clots, Calcareous Phlebolites, and Cholesterine." *Dr Douglas Maclagan* read a "Case of Preternatural Anus that discharged for 8½ years, and gave a minute account of the Pathological Appearances." *Dr W. T. Gairdner* "On the Morbid Anatomy of Cholera; he showed also some Specimens of Cancerous Disease of the Peritoneum;" *and* "a Specimen of Dissecting Aneurism of the Aorta," when *Mr Syme* expressed his belief as to "the cause of the Aneurism of the Aorta in the case of the late Mr Liston." *Mr Syme* read a letter from *Mr Ramsay, Surgeon at Broughty-Ferry*, as to the "Spontaneous Cure of an Aneurism of the Innominata and Arch of Aorta, notwithstanding its External Rupture." In 1850, *Dr W. T. Gairdner* read "On the Pathological Conditions of the Lung, connected with Bronchitis;" *also*, "Pathological Observations on Pericarditis and its Terminations;" *also*, "Account of the Morbid Appearances in a Case of Malignant Disease of the Suprarenal Capsule." In 1853, *Mr Syme* read a paper "On the Pedunculated Exostosis of the Long Bones;" and *Dr J. Y. Simpson* one "On Puerperal Arterial Inflammation and Obstruction." In 1854, *Dr Graham Weir* gave a "Case of Malignant Disease of the Pituitary

Gland," etc.; and *Dr Coldstream* read a comprehensive paper on "Catalepsy." In 1855, *Dr W. T. Gairdner* gave the details of the "*Post-mortem* of a Case of Aneurism, accompanied by Contraction of the Pupil on the affected Side;" *Dr Otto Spiegelberg* described "Tubercular Disease of the Lungs and Bowels, to which the cases of Tubercular Leprosy he had seen at Vienna had fallen victims;" *Dr W. T. Gairdner* gave a "Notice of Cases in which a Communication was established between the Stomach and the Colon;" and *Dr Haldane* gave an "Account of the Morbid Appearances in a Case of Chronic Hydrocephalus, accompanied by Cancer at the Base of the Brain." In 1856, *Dr Priestley* exhibited the "Urinary Organs in a Case of Chylous Urine;" *Mr John Barlow* communicated a "Note on the occurrence of Arterial Obstruction in the Horse, in connexion with Paralysis and Muscular Atrophy;" *Mr Spence*, "On the Necrosis of Scrofulous Patients, and its Relations to Caries;" *Mr Lister*, "On a remarkable Case of Hydrocele;" *Mr Miller*, "Case of Obstruction of the Pulmonary Artery;" *Dr MacCormac of Belfast*, "On Tubercle;" *Dr J. D. Gillespie*, "On Condyloma." In 1857, "On the Pathology of Inflammation," by *Dr Cappie*; "Notice of a Case in which a Tumour of the Pia Mater caused Compression of the Spinal Cord," by *Mr B. Bell*; "Case of Apoplexy from Plugging of a Cerebral Artery," by *Dr Laycock*; "On the Classification and Pathology of Morbid Growths," by *Dr Bennett*. In 1858, *Mr Lister* made "Remarks on a Case of Gangrene from Arteritis, and on the Causes of Coagulation of the Blood in Diseases of the Blood-vessels." *Dr M'Kidd of Elgin* gave a "Remarkable Case of Invagination of the Cæcum and Appendix." In 1859, *Mr William Turner, M.B.*, gave "Two Cases of Aneurism of the Descending Thoracic Aorta, producing Obstruction of the Thoracic Duct." *Dr Newbigging* gave the "History of the Case of the late Dr W. P. Alison." In 1860, *Dr P. H. Watson* exhibited a "Case of Villous Cancer of the Neck of the Female Bladder." In 1861, *Mr B. Bell* read a "Case of Perforation of the Stomach occurring under unusual circumstances;" *Dr T. Grainger Stewart* read a paper "On the Waxy or Amyloid Form of Bright's Disease;" *Dr W. Begbie* read a "Fatal Case of Croup in the Adult, exhibiting the parts affected;" *Dr Bruce* also read a "Case of Croup in the Adult;" and *Dr P. M'Laren* read, "Case of Pericœcal Abscess simulating Inguinal Hernia." In 1862, *Dr Begbie* communicated twenty cases of "Diphtheria and its Sequels; a Narrative." In 1863, *Dr Laycock* read, "On the Cerebro-Spinal Origin of Pulsations and Palpitations, and the Vascular Bronchocele termed Anæmic;" *Mr William Brown* read a "Case of Villous Cancer of the Bladder;" *Dr Warburton Begbie* exhibited "A Spleen about 11 lbs. in weight, from a Patient labouring under Leukæmia and Hypexanthine in the Urine;" *Mr Annandale*, "Gangrene of Foot and Leg depending on Obstruction of the Femoral Artery at the Groin;" *Dr T. G. Stewart*, "Further Observations on the Waxy or Amyloid Form of Bright's Disease." In 1864, *Dr Alexander Wood* read his "Notes of a Remarkable Case of Stricture and Dilatation of the Œsophagus, with Preparation;" *Dr T. G. Stewart* read a paper on the "Amyloid Degeneration;" *Dr Stephenson*, on the "Pathology of Rickets;" *Mr Spence*, "Case of Spontaneous Gangrene of the Lower Extremity, with a rare form of Arterial Lesion;" *Dr Alexander Simpson* gave a "Case of Death from Rupture of a Varicose Vein;" and *Dr Haldane* exhibited a "Case of Abdominal Phthisis," and also gave an "Illustration of Coal-miners' Phthisis." In 1865, *Dr Gamgee* read, "On the Character of the Expectoration in Two Cases of Fœtid Bronchitis and Gangrene of the Lung," *also*, "On the Physiology and Pathology of certain forms of Dilated Pupil;" *Dr Haldane* showed "Tubercular Caries of a Dorsal Vertebra;" *Dr Gamgee* showed a "Specimen of Tyrosine obtained from the Urine of a Patient suffering from Acute Atrophy of the Liver;" *Dr T. G. Stewart* read a paper "On Acute Yellow Atrophy of the Liver;" *Mr Annandale* exhibited a "New kind of Tumour which he proposed to call Vascular Recurrent;" *Dr T. G. Stewart*, "On Acute Atrophy of the Kidneys and Liver;" *Dr Watson* showed a "Case of Cystic Dilatation of the Epididymis and Vas Deferens;" *Dr W. C. M'Intosh* showed a "Dissection, Drawings, and Cast of a Case of Bronchocele." In 1866, *Dr Sanders* showed a specimen of "Purulent Phlebitis;" *he also* read a "Case illustrating the Supposed Connexion of Aphasia with Right Hemiplegia and Lesion of the Third Left Frontal Convolution of the Brain;" *Dr John Duncan* exhibited a "Specimen of Aortal Aneurism that had been treated by Galvano-puncture;" *Dr T. G. Stewart*, "Curious Case of Stricture of Duodenum;" *Dr M. Duncan*, "On Pelvic Serous Cysts following Puerperal Inflammation;" *Dr T. G. Stewart* showed "Syphilitic Masses in the Liver, illustrating Different Stages;" *also*, "Echinococcus Cysts in Liver and Nævus of Liver, in the same subject;" *Dr Tuke*, "Portion of a Brain in a Case of Aphasia;" *also*, "Portion of a Brain in a Case of Senile Dementia;" *also*, "Two Specimens of Complete Atrophy of the Cerebellum." In 1867, *Dr P. D. Handyside* read a paper "On Cartilages, Semi-detached and Loose, in Joints." In 1868, *Dr Bryson, Hawick*, communicated "A Fatal Case of Intussusception, occurring after two previous attacks;" *Dr Sanders* read a paper "On Heart Disease from Obstructed Coronary Arteries;" *Dr Bennett*, "On the Pathology and Treatment of Uræmia;" *Dr Aitken* gave a paper "On a Case of Gliomatous Tumour of the Brain;" and *Dr Sanders* one of "Heart Disease—Fibroid Degeneration, the result of Myocarditis." In 1869, *Dr Joseph Bell* read a "Remarkable Case of Perforating Ulcer of the Stomach;" and *Dr Chiene* showed a "Dissection of a Case of Double Herniæ;" *Mr Annandale* gave a paper "On Fatty Hernia;" *also*, a "Case of Unclosed Urachus, with

Umbilical Fistula." In 1870, *Mr B. Bell* gave a paper "On a Peculiar Paralytic Condition of the Lower Extremities following Gastric Fever;" *Dr J. Bell* read, "Observations on Rapid Pulmonary Congestion as an Occasional Cause of Death after Surgical Injuries and Operations;" *Dr T. G. Stewart* read, "Note of a Case of Waxy Degeneration, under observation for ten years." In 1871, *Dr J. Bell* read, "Notes of a Case of Osteoid Cancer;" *Dr T. G. Stewart*, "Inflammatory Bright's Disease, Fatal in the Third Stage;" *Dr Gillespie* read, "Notes of a Case of Extensive Fracture of the Skull and Vertebræ, with Extravasation into the Cord and Cilio-Spinal Region;" *Dr Taylor of Penrith*, "Case of Intestinal Obstruction from a Knot on the Ileum." In 1872, *Dr Tuke* read, "Case of Lesion of Broca's Convolution, accompanied with Amnesic Aphasia;" *Dr Charles Stewart of Denny*, "Notes of a Case of Temporary Hemiplegia, the result of Embolism;" *Dr T. G. Stewart*, "Case of Dilatation of the Bile-ducts;" and *Dr Gamgee* read, "Cases of Lymphoma observed in the Sick Children's Hospital, with Commentaries, Clinical and Pathological." In 1873, *Dr Tuke* read a paper "On the Morbid Histology of the Brain and Spinal Cord, as observed in the Insane, illustrated by Microscopes;" *Dr P. D. Handyside* "showed in the Brain of a Female, aged 34, Wagner's Convolutions and the Island of Reil of right side, both gone: there had been Hemiplegia and Amnesia, but no Aphasia (with a Drawing);" *Dr Gamgee* gave "Notes of a Case of Enlargement of the Spleen and Liver, with Hypertrophy of the Lymphatic Glands, terminating fatally by Hæmatemesis;" *Dr Stephenson* narrated "Cases of Laryngeal Abscess simulating Croup;" *Dr P. D. Handyside* gave a similar "Case, to which he had been summoned to perform Tracheotomy, and in which he only laid open a post-laryngeal abscess;" *Dr Affleck*, "Two Cases of Intussusception in Children;" *Mr Annandale*, "Tetanus cured by Removal of a Cicatrix, with portion of Injured Nerve;" and *Dr T. J. Maclagan*, "Uræmia and the Nervous Symptoms of Fever."

At the Conversazione, *Dr George Balfour* showed under microscopes "Several forms of Parasitic Plants, believed by Hallier to be always associated with Disease. The preparations were put up by Hallier himself, and comprised *two* forms of the Oidium Pestis (Rinderpest), *one* of Urocystis Oryzæ (Choleracultur, 70), and *one* of the Aeroconidia of the Parasite found in Scarlatina." *Dr Tuke* exhibited "(1.) Skull of an Epileptic Idiot, thirty-seven years of age; weight of brain, 60 oz.; hemispheres unequal—right 30¼ oz., left 23¼ oz. Patient had been a subject of left hemiplegia. Cerebellum deformed. Specimen shows unsymmetrical middle and posterior fossæ, deformed left foramen lacerum posterius, abnormal arrangement of sulci of longitudinal and lateral sinus, and nodule of bone on basilar process, caused by ossification of a prolongation of the basilar synchondrosis. (2.) Calvarium, showing new bone thrown out in a case of Cerebro-Spinal Meningitis. (3.) Skull with Deficient Sutures. (4.) Brain with Lesion of Broca's Convolution, without Aphasia. (5.) Under microscopes—(*a*), Amyloid Degeneration in Gray Matter of a Chronic Maniac; (*b*), Miliary Sclerosis in Paralytic Insanity; (*c*), Fuscous Degeneration of Cells of Superior Cervical Ganglion of a General Paralytic; (*d*), Deposit of Hæmatosin on vessel from Centrum Ovale of an Epileptic."

VII. MATERIA MEDICA.

Some original papers of mark appear early in our records. These describe *the appearance and the composition of Remedies, and of various Apparatus* useful in the practice of medicine. In 1822, *Dr James Home* exhibited to the Society "The Root and Flower of the medicinal plant Adra, from Jamaica." In 1824, *Mr Alexander Watson* read a paper "with reference to the Stethoscope;" and *Mr James Bryce* "showed a very simple and beautiful Contrivance of a Stomach-Pump, acting on the principle of a Syphon." In 1825, *Dr Alison* reported that "the most marked success" had in his experience attended the use of this syphon-pump. In 1828, *Dr John Davy* read "Observations on James's Powder and the Antimonial Powder (L.)." In 1836, *Dr Christison* read a paper "On the Composition, Adulteration, and Administration of Scammony;" and *Dr J. A. Robertson*, "On the Construction of a Portable Vapour-Bath at a small expense—Bath exhibited." In 1837, *Dr Christison* read, "On the Adulteration of Drugs; its Extent, Sources, and Prevention." In 1838, *Mr William Brown* read a "Notice respecting an Indian Root, the Indigenous Sarsaparilla, exactly agreeing in appearance, aroma, and medical virtues, with the American Smilax." In 1841, *Dr John Gairdner* read a "Notice of a Method of preparing Vegetable Extracts, with Specimens." In 1842, *Dr Graham Weir* read a "Notice regarding the Preservation of Vaccine Matter in small Glass Tubes, with Bulbs, hermetically sealed;" and *Dr John Gairdner* "mentioned that some years ago he had received from a French physician a number of Small Tubes capable of being readily filled with Vaccine Lymph by Capillary Action, and the contents in which could be obtained by blowing into the Tube." In 1848, *Dr Strethill Wright* wrote, "On several new Galvanic Batteries." In 1850, he gave a "Notice of a new Voltaic Battery for Medical Purposes." In 1853, *Dr Husband* read a paper "On the Method of preserving Vaccine Lymph in Capillary Tubes."

At the Conversazione, *Dr J. H. Balfour* exhibited the "Fossil Punctated woody tissue of Conifer, Australia;" also a "Cupping Gourd from Old Calabar," and a "Large Model illustrating the Anatomy of an Exogenous Stem, three years old." *Dr Maclagan* "showed a Stethoscope, as improved by Laennec, consisting of a boxwood cylinder, 12 inches long, with a narrow perforation from end to end, one extremity having a movable piece of conical form fitting into the cylinder. The instrument is made to screw into two halves for facility of transport. It weighs nearly 12 ounces." *Messrs Duncan, Flockhart, & Co., Mr Mackay,* and *Messrs Macfarlan & Co.,* exhibited a large number of improved Pharmaceutical Preparations, including the most recently used Syrups, Fluid Extracts, Granular Effervescing Preparations, and Chloroform; along with some eclectic remedies, Codeia, Narceine, Hydrochlorate of Apomorphia, Nitrate of Amyl, etc.

VIII. THERAPEUTICS.

Although Therapeutics, or the *modes of action of Remedies or Means* by which Nature may be furthered in her preservation of or return to health, can scarcely yet be said to be a distinct branch of science, or to have advanced much farther than when Montaigne forbade his wife to fetch a physician, in the words, "I am not strong enough for *that;*" or when Rousseau exclaimed, in presence of his doctor, "Laissez-moi mourir, mais ne me tuez pas":—and although our profession stands, perhaps, *now* on a higher platform than to be the butt and jeer of rhymsters—

"The King employs three doctors daily,
Willis, Heberden, and Baillie;
Three exceeding skilful men,
Baillie, Willis, Heberden;
But doubtful which most sure to kill is,
Baillie, Heberden, or Willis"—

nevertheless, by alliances like the Medico-Chirurgical Society, the ground has been cleared—

First, By the minds of educated men being now disabused of the ideas that not long ago were current as to the infallibility of *particular lines of treatment*—for even Horace Walpole may have spoken wisely when he said, "Gout is not a disease, but a remedy, and therefore 'tis absurd to try to cure it;" and the same remark, I venture to think, may be almost said of Cancer, of Hæmorrhoids, etc.

Secondly, The ground has been cleared by our Society having lent a not unimportant aid in expiscating and recording *facts,* which assist and enable us to estimate at their real value our present Remedial Measures; seeing that medical practice is to be learnt, not from books, or rules, or "on the square," but from practitioners bringing forward cases, stating their difficulties before this Society, and comparing notes one with another.

Thirdly, It is well known that our Society has co-operated in two signal *Therapeutical Movements*—the use of Anæsthetics by Inhalation, and the origination of Subcutaneous Injection. In regard to the former of these, the late Dr Symonds of Bristol, in his Presidential Address to the British Medical Association in 1863, expressed himself as follows:—"Were there no other discovery to stand out on the medical annals of the last thirty years than the anodyne use of ether and chloroform, it would be enough to make the whole suffering world, through unborn generations, look back to the intervening era with admiration and gratitude."

Among the Society's numerous contributions towards forming an estimate of the real value of certain Therapeutic Agents, the following résumé may be offered. In 1822, a "Letter was read from *Dr Anald of Jamaica*, stating the great virtue of the plant Adra as a Tonic and Febrifuge in Remittent and Intermittent Fever, in Cholera, etc." *Dr Thomas Anderson* communicated a "Case of the Successful Treatment of Traumatic Tetanus by Tobacco." *Dr Ch. W. Coindet of Geneva* gave "Relation of a Case of Hysteria assuming the form of Tetanus, treated by Injection of Opium into the Veins, accompanied with General Reflections on the action of Poisons." In 1823, *Mr J. P. Rind* read a "Case of Phrenitis successfully treated in India, by opening the Radial Artery;" and *Dr Begbie*, "Cases illustrative of the Sedative Powers of Datura Stramonium." In 1824, *Dr Renton of Madeira* read a paper "On the Efficacy of Opium in Mania;" and *Dr John Stevenson, Arbroath*, "Cases illustrating the Contagious Nature of Erysipelas." In 1825, *Dr Ulick Burke of Dublin* gave "Cases of Dysentery treated by Acetate of Lead, with Opium." In 1826, "Further Remarks on the use of Tobacco in Tetanus," by *Dr Anderson of Trinidad; M. le Doct. Colson, Paris*, "Researches as to the Action of Mercury, especially in relation to its Use in Venereal Complaints;" *Dr W. Gibson, Montrose*, "Account of the Epidemic Erysipelas of Montrose and Neighbourhood in 1822;" and *Dr Nichelmie of Nice*, "Observations on the Climate of Nice." In 1827, *Dr R. Huie*, "On the utility of large doses of Calomel in allaying the Irritability of the Stomach in Enteritis, Ileus, and Colic." *Dr A. Duncan, jr.*, stated "that he had given an extensive trial to the Madar, and found it a safe and efficient diaphoretic." *Dr John Wilson of Hull* read "Observations on the Natural or Spontaneous Cure of Syphilis." In 1829, *Dr John Storrer of Nottingham* read "Some Account of a Cerebral Affection as arising during the course of Pulmonary Consumption, and arresting further progress of that fatal Disease;" and *Dr William Young*, "Remarks on the Effects of the Secale Cornutum in Cases of Tedious Labours." In 1831, *Dr Abercrombie*, "Case of extensive Dropsy, with a new Diuretic;" and *Dr Gairdner*, "Cases illustrative of the Deleterious and Remedial Powers of the Rhododendron Ponticum." In 1832, *Dr Alison*, "Notice respecting the advantages of Quarantine Houses in checking the diffusion of Cholera;" *Drs Latta and Craigie of Leith, and Dr John Mackintosh*, "Cases of Cholera in which the infusion of Saline Solutions into the Veins has been practised." In 1833, *Dr Alison*, "Notice of two Cases in which Nutritive Enemata were successfully employed." In 1834, *Dr John Smith*, "Account of Dysentery as it occurred in the Edinburgh Charity Workhouse, during 1832 and 1833, and where Calomel in large doses was found highly useful." In 1835, *Mr John Stuart of Kelso*, "On the Medicinal Action of Veratria." In 1836, *Mr P. C. Gibson*, "Notice of Cases of Puerperal Fever treated without Bleeding, with Remarks;" and *Mr W. Kerr of Paisley*, "On the Medicinal Effects of some of the Metallic Nitrates;" and *Dr J. Y. Simpson*, "Cases illustrative of the occasional Contagious Propagation of Pestilential Cholera." In 1838, *Dr P. Darling Veitch*, "Observations on the Medicinal Properties of Euphorbium." In 1840, *Dr Robert Hartle of Trinidad*, "On the use of Piperine in the Treatment of Intermittent Fevers." In 1841, *Mr W. A. F. Browne of Dumfries*, "Observations on Bloodletting in Mania;" *Dr Ransford*, "Cases illustrating the Use of Tincture of Cantharides in Infantile Bronchitis;" and *Dr W. Henderson*, "Cases illustrative of the Treatment of Rheumatism with Opium." In 1842, *Dr Dunsmure*, "Notice regarding the Use of Extract of Senecio Jacobea in Gonorrhœa;" *Dr Graham*, "Notice regarding the Allocation of Fever Patients in Hospitals;" *Mr James Miller*, "On the Treatment of the Hæmorrhagic Diathesis;" and *Dr (now Sir John) Cormack*, "Contributions towards an Estimate of the Real Value of Creosote as a Therapeutic Agent." In 1843, *Dr A. B. Stout*, "On the Cure of Inveterate Pannus by the Inoculation of the Matter of Blenorrhœa;" *Dr Alex. Watson*, "On the Process of Nature by which Hæmorrhage is stayed or prevented, and by which the permanent Closing of Wounded Arteries is accomplished;" *Mr Thomas Beaumont*, "On the Physiological, Therapeutical, and Pathological Effects of Alcohol;" *Dr Douglas Maclagan*, "On the Medicinal Qualities of the Bebeeru Bark of British Guiana;" and *Dr P. D. Handyside* gave his "experience of the rapid Cicatrization of Lupus by the Repeated Leeching of the Cavities of the Sores." In 1845, *Dr Bennett*, "On the Anatomical Evidences of the frequent Spontaneous Cure of Phthisis Pulmonalis, and the Indications furnished by Pathology for its Rational Treatment;" *Dr George Wilson*, "On the Employment of Oxygen Gas as a means of Resuscitation in Asphyxia, and otherwise as a Remedial Agent;" and *Dr Peddie*, "Cases illustrative of the Contagious Nature of Puerperal Fever and its connection with Erysipelatous Inflammation." In 1846, *Dr Scott*, "On the Non-Mercurial Treatment of Syphilis;" *Dr Christison*, "On Continued Fever originating in Local Miasma;" *Dr Sellar*, "On the Sedative Effects of Astringent Substances;" and *Dr Martin Barry*, "Case of Poisoning with Opium, in which Galvanism was successfully employed." In 1847, *Dr Christison* gave an "Account of an Epidemic Scurvy which appeared in the General Prison at Perth in 1846;" *Dr Alexander Wood*, "Case of Puerperal Convulsions following Delivery, where the Vapour of Ether had been inhaled during Labour;" *Dr George*

Balfour, "Notes on the Practice of Skoda;" *Dr J. Y. Simpson*. "Does Etherization Increase or Diminish the Mortality attendant upon Capital Operations in Surgery?" *Dr Tait*, "Observations on Opium-eating," and again, "Does Opium-eating affect the Duration of Life?" and *Dr J. Y. Simpson*, "Historical Notes regarding the Superinduction of Insensibility to Pain in Surgical Operations." In 1848, *Dr Wright*, "Description of and mode of applying Electro-magnetic Machine;" and *Dr J. Y. Simpson*, "On Solutions of Gutta Percha, Gun Cotton, etc., as Dressings for Wounds, etc." In 1849, *Dr Bennett*, "Case of Spontaneous Cure of Ovarian Dropsy;" *Dr William Keith*, "On the Safety and Suitableness of Chloroform as an Anæsthetic Agent in some of the more complex and serious Operations in Surgery;" and *Dr Warden*, "On the Diving-bell as a Medical Expedient, illustrated by an account of its Curative Effects upon a *Deaf* Patient." In 1850, *Dr Guggenbühl* gave "Some Account of the Present State of Cretinism in Switzerland, and of the Progress recently made in its Treatment on the Abendberg." In 1851, *Dr George Hamilton Bell*, "The Treatment of Erysipelas by the Tincture of the Muriate of Iron;" *Dr Charles Bell*, "with Additional Remarks and Cases;" and *Dr W. T. Gairdner*, "Notice of a Portable Clinical Microscope." In 1852, *Dr Bennett*, "On the Treatment of Continued Fever by Large Doses of Quinine." In 1853, *Dr Bennett*, "On the Curative Treatment of Phthisis Pulmonalis, and on the *modus operandi* of Cod-liver Oil—What is the experience of Edinburgh Practitioners' in regard to the Therapeutic Action of Cod-liver Oil in Phthisis?" *Dr J. Y. Simpson*, "On the possible Prevention of Consumption and Scrofula by Oil Inunctions and Inhalations;" *Dr John Struthers*, "Considerations on Local Bloodletting in Affections of the Internal Viscera;" *Dr William Scott*, "Practical Remarks on the Mineral Waters of Homburg;" and *Dr Traill*, "Cases illustrative of the Contagious Nature of Epidemic Cholera, as it has recently appeared in Arbroath." In 1854, *Dr J. Y. Simpson*, "Note on the Therapeutic Action of the Salts of Cerium." In 1855, *Dr Alexander Wood*, "On a New Method of Administering Medicines, more especially applicable to Painful Local Affections;" *Dr W. T. Gairdner*, "Notes on the Administration of Gallic Acid, chiefly in Bright's Disease and in Hæmoptysis;" and *also* "Verbal Communication as to the Diffusion of Cholera in the Remote Districts of Scotland;" and *Mr Benjamin Bell*, "A few Remarks on Dilution as a Principle in Therapeutics." In 1856, *Dr Alison*, "Reflections on the Results of Experience as to the Symptoms of Internal Inflammation, and the Effects of Bloodletting, during the last forty years;" *Dr Mackenzie, Kelso*, "Notice of a Case of Death during the Administration of Chloroform;" *Dr John Struthers*, "On Jugular Venesection in Asphyxia;" and *Dr John Chisholm, Dumfries*, "Observations on the Diuretic Action of Digitalis." In 1857, *Dr Bennett*, "Observations on the Results of an Advanced

Diagnosis and Pathology applied to the Management of Internal Inflammations, compared with the Effects of a former Antiphlogistic Treatment, and especially of Bloodletting; *Dr Christison*, "On the Changes which have taken place in the Constitution of Fevers and Inflammations in Edinburgh during the last forty years;" *Dr Alison*, "Reply to Dr Bennett's 'Observations on the Results of an Advanced Diagnosis and Pathology in the Treatment of Internal Inflammations;'" *Dr George Mackay*, R.N., "Notice on the Cholera which appeared at Varna in the year 1854, and more especially in H.M. ship 'Agamemnon,' in the Black Sea, between the 1st August 1854 and 8th September 1855;" *Dr Bennett*, "Correspondence sent to the Secretary by Dr Alison, containing opinions in reference to the points in controversy between him and Dr Bennett;" *Dr W. T. Gairdner*, "Communication on the same subject;" *Dr Budd of Bristol*, "Case of Acute Meningitis treated by early Bloodletting;" and *Dr Peddie*, "On the desirableness of some Legalized Arrangements for the Care and Treatment of Dipsomaniacs. In 1858, *Dr Pinkerton*, "The Causes of Cholera in the Crimea;" *Dr Boeck of Christiania*, "On Syphilization; its Mode of Performance, and its Results;" *Dr Begbie*, "Notes on Arsenic; its Physiological and Therapeutical Effects;" *Dr Christison*, "On the Changes which have taken place in the Constitution of Fevers and Inflammations in Edinburgh during the last forty years (Part Second);" *Dr Matthews Duncan*, "Case of Infantile Convulsions successfully treated by Carbonate of Iron, after failure of other means;" and *Mr Benjamin Bell*, "The Therapeutic Relations to each other of Opium and Belladonna." In 1859, *Mr Charles Sidey*, "Case of Aneurism of the Femoral and Posterior Tibial Arteries; Spontaneous Cure." In 1860, *Dr Alexander Wood*, "Smallpox in Scotland, as it Is, Was, and Should be: some Suggestions for a Vaccination Bill;" *Mr Little of Singapore*, "On Exposed Coral Reefs as a Cause of Fever in various parts of the East;" *Mr William Brown*, "On the Results of Re-vaccination at the Orphan Hospital;" and *Dr Thos. Anderson, Bengal Army*, "On Opium as a Remedy in Poisoning by Datura." In 1861, *Dr W. T. Gairdner*, "On the Use of Alcoholic Stimulants in Hospital Medical Practice; with Illustrations from the Records of the Royal Infirmary;" and *Dr M'Kidd of Elgin*, "On the Therapeutic Effects of Ipecacuan in Diarrhœa and Dysentery." In 1862, *Dr Arthur Mitchell*, "Marriages of Consanguinity, their Influence on Offspring;" and *Dr J. D. Gillespie*, "Epidemic of Scarlet Fever at Donaldson's Hospital during the Winter of 1861-2." In 1863, *Dr D. Argyll Robertson*, "On a New Agent in Ophthalmic Medicine." In 1864, *Mr Spence*, "Some further Observations on the Employment of Tracheotomy in Diphtheria and Croup;" and *Dr Stockwell, Musselburgh*, "Case of Spontaneous Evolution of a Foreign Body through the Parietes of the Abdomen." In 1865, *Dr Clouston*, "On an Epidemic of

Dysentery in the Cumberland and Westmoreland Asylum, due to Sewage Miasm;" *Dr George Balfour,* "Cullen and Gregory on Change of Type in Inflammation;" *Dr W. Stephenson,* "On Pepsine in Diseases of Children;" *Dr John Smith, jun.,* "On the Action of Medicinal Preparations of Iron on the Teeth;" and *Dr Gillespie,* "A Case of Sudden Death while under the Influence of Chloroform." In 1866, *Dr John Smith, jun.,* "Notes on some Points in the Administration of Chloroform; *Dr Warburton Begbie,* "On Paracentesis Thoracis in the Treatment of Pleural Effusions, Acute and Chronic;" *Dr A. J. Spence,* "On the Mode of Action of Strychnia;" and *Dr Junor, Peebles,* "Case of Traumatic Tetanus successfully treated by the Local Application of Infusion of Tobacco." In 1867, *Mr Ramsay, London,* "On the Adaptation of Vulcanized Indiarubber to the Mechanical Treatment of Deficiencies in the Hard and Soft Palate;" *Dr E. Watson,* "On the Treatment of Traumatic Tetanus by the Calabar Bean;" and *Dr Stephenson,* "On the Action and Uses of Phosphate of Soda in small doses." In 1868, *Dr Arthur Mitchell,* "The Bodily and Mental State of the Pregnant and Nursing Mother in connexion with Idiocy in her Offspring;" *Dr John Duncan,* "On a Case of Epilepsy connected with Vesical Calculus, cured by Lithotomy;" *Dr A. G. Miller,* "A Case of Disease of the Spine successfully treated with Carbolic Acid;" *Dr George Balfour,* "On the Treatment of Aneurism by means of Iodide of Potassium;" *Dr Lombard of Geneva,* "On the Mortality at different Seasons—illustrated by Maps;" *Dr Thin of Shanghai,* "On Cholera at Shanghai;" and *Dr Andrew Wood,* "The Question of the Site of the New Royal Infirmary," and "Adjourned Discussion on the Question of the Site of the New Royal Infirmary." In 1869, *Dr Halliday Douglas,* "On the use of Indian Hemp in Chorea—a Case;" *Dr Joseph Bell,* "Cases illustrating the Use of Carbolic Acid as an Antiseptic Dressing;" *Mr Alex. Macarthur, Anstruther,* "Case of Traumatic Tetanus—Recovery under the use of the Calabar Bean;" *Dr P. D. Handyside* gave a "Case of Spontaneous Closure of an Umbilical Fistula, extending 2 inches along a partially closed Urachus, in a Male Infant;" and *Dr Brown, Carlisle,* "Case of Tetanus treated by Bromide of Potassium." In 1870, *Dr Gillespie,* "Notes of an Epidemic of Typhoid Fever at Donaldson's Hospital;" *Dr Bennett,* "Notice of the Therapeutic Effects of Chloral," and *also,* "On the Injurious Effects which may follow the use of Mercury in Hepatic Diseases." In 1871, *Dr Rutherford, Dep. Insp.-Gen.,* "Remarks on Dr Bennett's Case of Hepatic Abscess, said to have been treated by Mercury;" *Dr T. R. Fraser,* "Sketch of the Present State of our Knowledge respecting the Action of Mercury on the Liver;" *Dr Stephenson,* "The Action of Mercury in Children;" *Dr Gillespie,* "Case of Unreduced Dislocation of the Humerus of seven weeks' Duration; Death under Chloroform;" *Dr M'Kendrick,* "Experimental Notes on the Action of Aloin;" *Dr W. Begbie,* "The Therapeutic Actions and Uses of Turpentine;" *Dr George Balfour*, "Notes on the Treatment of Diabetes by Lactic Acid (Cantani's Method);" *also*, "Notes of a Case of Enlarged Spleen, successfully treated by Inunction of Biniodide of Mercury Ointment;" and *Dr Stephenson*, "On Electro-Therapeutics." In 1872, *Dr Fergus* of Glasgow, "The Sanitary Aspect of the Sewage Question, with Remarks on a little-noticed cause of Typhoid and other Zymotics;" *Dr Maclaren*, "Notes of a Case of Pleuro-Pericarditis, in which Paracentesis Pericardii was performed;" *Dr Affleck*, "On the Treatment of Dilatation of the Stomach by the Method of Kussmaul, with Cases;" *Dr Warburton Begbie*, "The Therapeutic Actions of Muriate of Lime."

At the Conversazione, *Dr J. H. Balfour* exhibited "(1.) A Series of Models of Esculent and Poisonous Fungi, including amongst the former *Agaricus campestris, A. proceris, Boletus edulis, B. scabra*, and *Cantharellus cibarius.* The latter included *Boletus luridus, B. satanus*, and *Amanita muscaria*, all of which are highly narcotic. *A. muscaria* produces intoxication, delirium, and death. (2.) A Collection of Dried Specimens of Edible and Poisonous British Fungi. The plants were collected and dried in 1868, and yet retain all the freshness of the pileus and stipe. (The specimens were waxed, so as to prevent them absorbing moisture.) (3.) A Collection of Wax Models of Minute Fungi, which cause disease in other plants, including *Polystigma rubrum, Valsa pulchella, Trichobasis geranii, Uncinula adunca, Ræstelia lacerata, Vermicularia trichella, Arcyria puncea*, and *Aspergillus glaucus.* (4.) Under six compound microscopes were exhibited Microscopic Fungi; 'Tyloses' in vessels of Vine, and other preparations."

Dr Douglas Maclagan exhibited, "in action, an Anemometer, by Casella, for estimating the ventilating currents in Rooms, Hospital wards, etc. The instrument is furnished with an index and pointer, by which the rate of movement of air per minute can be readily calculated. On various occasions during the evening, the Anemometer was exhibited in action at one of the side doors of the hall, when it was found that the room was being in some measure ventilated by a current of cold air passing in near the floor, whilst an opposite current of warm air passed out above."

Dr Joseph Bell exhibited "a Dieulafoy's Aspirateur Pneumatique. This instrument consists of a strong exhausting syringe, connected with two tubes. By it internal abscesses can be safely and easily emptied, the bladder can be tapped, and it is a most safe and excellent means of diagnosing the contents of deep-seated fluid Tumours, Cysts, or Abscesses, being very useful, also, in the case of doubtful Tumours."

Dr George Balfour exhibited "M. Paul Reynard's Aspirateur, made by Mathieu of Paris, working by a steam vacuum, which, from practical experience of both, is much superior to Dieulafoy's for Thoracentesis, for Paracentesis Abdominis, and for the evacuation of the fluid in Hydatid Tumours of the Liver; acting well, and not taking more time than that indicated by M. Reynard, while its easy and painless employment and the automatic way in which it acts as a syphon, without any risk of the entrance of air, make it a great improvement over former methods, nor even excepting Bowditch's syringe."

Dr Stephenson showed "an Electric Battery for the use of the Constant Current, by Weiss, London. Contains thirty cells (Smee's). When the lid is shut, the cells with acid are lowered from the plates. When the lid is open, they are raised, and the battery is in action."

Dr Dumbreck exhibited an "Ancient Chinese Magnetic Compass, set in a circular *boxwood frame minutely graduated*, and 4 inches in diameter."

Mr Andrew Davidson, Medical Missionary, Madagascar, exhibited a "Specimen of the Bark of Landema—a species of Cinchona—used in Madagascar as a Tonic and Antispasmodic."

(*To be continued.*)

Part Second.

REVIEWS.

The Obstetrical Journal of Great Britain and Ireland. London: J. and A. Churchill: 1874.

The American Journal of Obstetrics and Diseases of Women and Children. New York: Wm. Wood and Co.: 1873.

Archives de Tocologie, Maladies des Femmes et des Enfants Nouveau-nés. Paris: Adrien Delahaye: 1874.

Annales de Gynécologie. Paris: 1874.

Now that the first volume of the London Journal of Obstetrics has appeared, we are led to take it as a whole into consideration, and we are glad to say that it appears to us to have made a fair start. It has made its bow, and certainly deserves a favourable reception. We cannot say more, as we would have been glad to do. All we say is—Good, for a beginning. Its prosperity, even its continued existence, are not yet assured by its quality, but there is good reason for hope.

At present the journal depends almost exclusively on its original communications. Of these, several are good, most of them very poor, none of first-rate quality. We hope our obstetricians will work, and send their work to this journal, so as to make it stand well beside its compeers in Germany, France, and America.

The editor should, however, have some space devoted to reviews, and we hope he will not admit those "buttery" notices by ignorant and blunt or time-serving men, which deface the pages of some of the weeklies. A review should be written by a well-informed man, and should be fearlessly composed, yet always with consideration and kindness. All books should be treated alike, whoever may be the author, for we are not quite sure of the justice of that canon of criticism, which recommends unsparing rigour only in the case of great authors.

We observe the editor occasionally annotates an original article—a proceeding which we regard as generally improper, and seldom justifiable. If the practice is continued, it will drive away sensitive contributors, whose property is thus imprudently dealt with. We observe also with some pain, that prominent place is given to controversial epistolary trash from Barnes, Snow Beck, etc.—a kind of contribution which the editor should either entirely decline, or cram into some small-type out-of-the-way corner.

We call the attention of our readers to the American journal because, since Dr Munde has become associate editor, a new and very valuable feature has been added to it. This consists in the translation, from foreign and especially German journals, of a selection of the best papers. By this simple means the value of the journal, to all really knowing, and interested in, the progress of obstetrics is immensely enhanced. The papers translated are not only well chosen, and in themselves of great importance, but they are good samples of the style and method of article-writing; and, as models, are sure to do good. Of course the originals are German.

We commend the example of this journal to our other obstetric editors.

The first of the French journals is remarkable as the fourth or fifth which has started within a very short period of time. Of these some have died a timely death, others will follow. The "Archives de Tocologie," although well edited—Depaul, Stoltz, Bernutz, and Hervieux being on its staff—is still only making a start; we are sorry we can scarcely say, a fair start. The articles are of that talky-talky description which a clever fellow can write without stopping, or the fear of it. We find little that can be called substantial, or the fruit of good patient work. We wish this journal prosperity. The French are a great nation, and quite able to hold their own in obstetrics if they set about it with zeal in the right way.

We are sorry to see the Annales de Gynécologie, etc., because it indicates division in the camp, which is much to be deplored for many reasons, and among them because France does not (we do not say, cannot) produce matter for two journals. The remarks we have made on the French Archives apply to the Annales. The papers are poor, and do not indicate any careful work. The best of both journals combined would not make one good one. Why should not Pajot, Gallard, and Courty, of the Annales, join with Depaul, and Stoltz, and Bernutz, of the Archives, and conspire to make one good journal? We hope that, first, they and their confrères will work, for without this they should stop both journals; and we hope that, second, they will make up their differences, and publish only one good journal. In a like spirit we speak when we express a hope that the Boston Gynæcological Journal has expired. There is room and to spare in the American Journal of Obstetrics for three times as much work as is done in America at present. We are sorry to observe that the Berlin Beiträge has stopped, for that was a good journal, containing many excellent papers; and we are still sorrier about it when we observe that it is probably the result of division in the camp there, the great Berlin Obstetrical Society having been broken up from internal dissensions, and a rival society started under the presidency of Martin. This is a great calamity, and we hope that in Berlin the divisions will soon cease and good feeling prevail. The London Obstetrical Society is also somewhat shaken at present. The question of medical women, although most seen, is not the weightiest for it. Its thriving will be secured only on condition of its dispensing impartial justice, shaking off the influence of individuals or of cliques, and, above all, its discouraging all forms of quackery, and encouraging only good honest workers and thinkers, a class of which it has only few specimens.

To all these journals we unhesitatingly say,—Look at the "Archiv für Gynækologie." There is an example for you. Follow it. We would show to the editors of the above-noticed journals, that of this great German journal there is not one number to be found that does not contain papers of a kind that merits, if it does not ensure, success. There is as much real work in a single number of the German Archiv as in a volume of any of the other journals.

Lessons from the Lives of Irish Surgeons: An Address introductory to the Session of the Royal College of Surgeons, Oct. 1873. By E. D. Mapother, M.D., Professor of Physiology, etc. Pamphlet, pp. 21. Dublin: Falconer: 1873.

This is an appreciative and interesting lecture, somewhat fragmentary and discursive in its style, but going over a great deal of ground and conveying much information about the worthies of the Dublin School during the last hundred years. Bold, warm-hearted, generous, and witty, these great Irish surgeons must have been not only ornaments to the profession, but thoroughly pleasant companions and faithful friends.

Report of the Sanitary Commissioner for Madras, 1871. Madras: 1872.

Note on the Epidemic Connection of the Cholera of Madras and Bombay, with the Cholera Epidemics of the Bengal Presidency. By JAMES L. BRYDEN, M.D., Surgeon, Bengal Army, Statistical Officer attached to the Sanitary Commissioner with the Government of India.

Report on the Cholera Epidemic of 1872, *in Northern India.* By J. M. CUNINGHAM, M.D., Surgeon-Major, Bengal Medical Service, Sanitary Commissioner with the Government of India. Calcutta: 1873.

WE are sorry so much time has gone by before we could review the works which Mr Cornish and Dr Bryden have sent us, but the task is to us no easy one. We have to weigh contradictory statments, often put forth with great confidence, about occurrences in a wide and distant country. Our anxiety is not lessened by the reflection that these occurrences bear upon a very important subject, on which it is often necessary to come to some practical conclusion.

The main question in Mr Cornish's Report is the vexed one of the propagation of cholera, but we have also a collection of statistics upon the general health of the Madras, European, and native army; of the jail community, and the civil population in general.

The strength of the European army in January 1871, was 11,053; their mortality was 20·82 in the thousand; but double that number was invalided, 29·86 per thousand being sent away for change of climate, and 12·54 being discharged. The strength of the native army was 31,386; the deaths were 11·08 in the thousand, the invaliding 2·23. Under this latter head were included those who had served out their time.

The number of the jail population during the year 1871 was 27,058; the deaths 17·9 in the thousand, showing a considerable diminution of mortality since previous years.

Mr Cornish is disposed to doubt the statement brought before Parliament, that soldiers under twenty years of age are more liable to sink under the diseases of India than after that age. Recruiting is going on so slowly in Britain that this is a matter of great importance, and it may be well to add Mr Cornish's own words:—

"I have looked in vain for any *reliable* statistics, showing that the men under twenty years of age are more liable to disease or early death than those who arrive in India in the next quinquennial period of life. The information, as regards sickness, can only be obtained from the admission and discharge books of each corps or brigade, and, until it is furnished, those who are the responsible sanitary advisers of the Indian Governments are left to surmise that the very important question of the age at which recruits may be safely sent to India has been disposed of in the absence of those statistical details, which alone could yield the proper basis for a sound judgment."

Mr Cornish has made some observations on the prevalence of malaria on irrigated lands. As far as his examination goes, he cannot find any proof that ague is increased by the use of irrigation. As irrigation will undoubtedly extend itself in India, this is pleasing intelligence. The people of the Godavery district have, he says, "peculiar views on the treatment of fever, preferring to abstain from all food for a period of ten days; and in carrying this theory into practice, a good many die of exhaustion. The ordinary fever of the northern districts, in the cold season, is a quotidian intermittent, attended often by gastric irritation. The fever is amenable to proper treatment, but under the starving system, which is generally pursued, it causes much mortality." We hope in time to have some observations from Mr Cornish and Dr Cuningham on the efficacy of plantations of the Eucalyptus globulus in counteracting malaria.

To come now to cholera, Mr Cornish stands up vigorously against Dr Bryden's theory, that cholera is mainly propagated by an air-borne miasma generally following the course of the monsoons. He insists that there are repeated examples in which cholera has travelled through the Madras Presidency and into Ceylon, against the course of the prevailing winds; and he states that there is no proof that an invading epidemic ever covers a whole district *per saltum*, as Dr Bryden and others have maintained. He supports his statements by long and circumstantial details.

Mr Cornish takes occasion to reply to the statements which Dr Bryden, in the pamphlet at the head of the article, has made in reference to his work "Cholera in Southern India." In his note on the epidemic connexion of the cholera in Madras and Bombay, Dr Bryden cites a number of authorities to disprove the assertion that cholera habitually advances against the prevailing winds. None of his authorities are decisive; and he may be fairly challenged for assuming that a deficiency of rain may be taken as a proof of the absence of the winds which bring the rain; for sometimes the clouds come and no rain falls, and the deficiency of rain may be owing to the very violence of the winds. On the other hand, we are not at all sure that the movements of the winds have been determined with the certainty which Mr Cornish assumed to have been the case, and would rather wait for renewed observations than declare this part of Dr Bryden's theory finally disposed of. Dr Bryden insists that although it is difficult to make out how the winds blew and kept on blowing during bygone epidemics of cholera, in general the facts harmonize with his theory, and he proposes that the phenomena of the advance of cholera should in future be mapped out, and its progress in relation to the winds more carefully observed. "I am well aware," he writes, "that it is difficult to put on one side, even for a time, the strongly impressed conviction that the movement of cholera cannot take place without the intervention of man.

"The study of three fundamental laws will, however, tend to lessen or remove this difficulty—

"First, the law of the succession of epidemics. This teaches the doctrine universally admitted in India, that in what we recognise as a new epidemic, it is a fresh material which invades. And the special antagonistic doctrine which it opposes, is that which suggests that it may be an old soil-sown cholera, possibly derived from man, which is drawn out from the earth by a special meteorology.

"Second, the law of progress in directions which are definite, and which are not those of human currents. Whether these currents are aerial currents, as I maintain them to be from the consistent history of the past fifty years, is a matter of observation; the radical point to be grasped in connexion with this law of progress is, that epidemic cholera in its great movements does nót follow routes of human intercourse—a doctrine which is in accord with all Indian experience."

"When I say that cholera appeared epidemically in 1869 from the Runn of Cutch to the slopes of the snowy Himalayas bordering on Thibet in the same week; when I say that the first case marking the invasion of the Punjab occurred in the same week; when I map down definitely the fact that Central India was covered universally in the same week; when I show another great and definite tract covered on the 18th August in the same year, and a further projection in the first week of September, culminating on the same day throughout the Peshawur valley and in Southern Scinde,—I state a fact and no theory. It is no imperfection of our information which determines that I shall thus place on record as true what is substantially incorrect, however beautiful it may be in theory. The terrible epidemics of 1856 and 1861 in Northern India had each the same distribution and limit to a mile, the same day and date of invasion, and the same phenomenon of an instantaneous leap into the unoccupied area; and it is no mere theory that such was the case. The phenomena of every year and every epidemic are calling for the recognition of the same truth—a truth which may be contradicted, but cannot be controverted."

We scarcely think the reader will blame us for not coming to a definite conclusion upon statements the original evidence of which is not accessible to us, and in which two competent authorities differ in so marked a manner. When Dr Bryden says that Hecker's Great Black Plague of 1348 was the cholera which came from India, we know well enough that it was the Egyptian plague, and not the cholera, because books are accessible to us which are very likely rare in India. Nevertheless, we do not doubt either the value of the statistics in the hands of Dr Bryden, or his fidelity in rendering them. As long as they are denied by such observers as Mr Cornish, they must be doubtful to us; but the winds will come again, and, in the process of time, decide the question one way or another.

In the Madras Report, we have the details of some observations made in the Madras Presidency, to test the theory of Pettenkofer, upon the influence of subsoil moisture on cholera, carried on at six stations of the presidency. The observations are not regarded by Mr Cornish as complete nor decisive, but as far as they go they do not confirm Pettenkoffer's theory, which, in our opinion, is more skilfully framed to keep out of the range of negative facts than to rest upon positive data.

Mr Cornish gives the details of his examination about the epidemic of cholera, which broke out so suddenly amongst the 18th Hussars at Secunderabad, and which carried off thirty-three men, two women, and four children, in ten days; and in which fifty-three persons were attacked within forty-eight hours. We have no intention of going over all the evidence and counter-evidence on this point. To do so fairly would require several pages of the *Journal*. Mr Cornish believes that contamination of the water-supply of the 18th Hussars with choleraic matter affords the best explanation of this sudden, virulent, and isolated outbreak, and so far we are disposed to agree with him; but this explanation, in the present state of our knowledge of the question, goes no further than a presumption. Mr Cornish shows that there were native travellers affected with cholera in the neighbourhood, and that they may have contaminated the water-supply of the 18th Hussars, but he does not even pretend that he can prove this to be the case; and Deputy Inspector Cowen, and Drs Scott and Anderson, who also examined the question, and made separate reports upon it, are all of opinion that the water-supply of the Hussars was not contaminated by choleraic dejecta, and that the water they used was drunk by other people among whom no cholera appeared. The reports of these officers are given in the appendix.

Mr Cornish has, it is true, the last word; and states that his surveys led him to disagree with their conclusions; but as we have not the pleasure of knowing any of these gentlemen, we are by no means disposed to say that one man must outweigh three. If the genesis of cholera were fairly admitted beyond dispute, the details and presumptions furnished by Mr Cornish might be thought quite sufficient; but as long as the matter is *sub lite*, we do not think that disputed assertions should be treated as indisputable proofs of theories still doubtful, as has, in this case, been done by some periodicals.

It is instructive to compare the history of the Secunderabad outbreak with a similar one recorded by Dr Cuningham in Bengal.

"The history of the very violent outbreak in St Peter's College at Agra, the most severe known in the whole course of the epidemic, is just one of those cases that should be explicable on the water theory, if the water theory is correct. But it admits of no such explanation. The water used by the boys was drawn from a well common to the College and a considerable native community, among whom, so far as is known, no cases occurred. It may be argued that, though nominally drawn from this source, in reality it was very likely taken from the College well employed in former years —a well in most dangerous proximity to the old latrine, which is practically a well of filth close beside it. But this explanation accords with the facts no better than the other; for the day-boarders, twenty-seven in number, who drank of the same water as the boarders and orphans, and drank largely too—as I was assured, and as might naturally be expected in that very hot weather—all escaped, with the exception of one. Anxious to ascertain the circumstances under which this boy was attacked, I found, on inquiry, that he was the only one of the day-scholars who lived close by—a fact which points strongly to localization, not to water.

"So far the argument has been pursued, as if it were a fact established beyond all dispute that man multiplies within himself the poison of cholera, and that the discharges contain that poison. But few will admit that these statements have really been established as facts. At the best they are but assumptions, in support of which there is very little proof. The history of the epidemic of 1872 certainly lends no countenance to them. Notwithstanding the widespread belief in this water theory, it is a very remarkable fact that in India, where cholera is so prevalent, even in Lower Bengal, where it is always present, no case has ever been adduced in which there is good reason to believe that water contaminated with cholera evacuations really has produced cholera. It would be a mere waste of time to discuss any supposed instance of this nature in which the facts are not most fully and circumstantially related."

Dr Cuningham's Report on the Cholera Epidemic of 1872 in India, must be disagreeable reading for those who have committed themselves to an avowed belief that cholera propagates itself mainly through contagion, or mainly through the contamination of drinking water. As Sanitary Commissioner with the Government of India, Dr Cuningham has had unusual opportunities of studying the diffusion of cholera on the largest scale; nor can it be said that he had any prejudice against the theory of contagion. On the contrary, in his Fourth Annual Report, reviewed by us in the *Edinburgh Medical Journal* for July 1869, he seemed inclined both to favour the view that cholera is diffused by contagion, and that it may be checked by quarantine. Prolonged study and consideration has now induced Dr Cuningham to come to the conclusion that the diffusion of cholera in India cannot be explained by either of these theories. The reasons which have induced him to adopt this conclusion are given at length, and supported by a long array of evidence.

Dr Cuningham states his disbelief that the great outbreak of cholera in 1867, following upon the fair of Hurdwar, had anything to do with the pilgrimage, save in so far as exhausted travellers are peculiarly liable to be visited by an epidemic. From 1867 to 1873, the annual fairs have passed without any sickness save a few isolated cases. Indeed the fairs at Hurdwar seem, on the whole, to have singular exemption from the visitation of the disease. "Cholera," he observes, "attacks large areas, sweeping along one continuous tract, and sparing adjacent districts. There is no slow advancing spread. Immense tracts suffer in common." This confirms Dr Bryden's views, that cholera spreads *per saltum*.

The disease does not travel more rapidly than it used to do when there were no railways. As has been over and over again pointed out and never replied to, the attendants upon cholera patients are not oftener attacked than other people. Dr Cuningham remarks:—"I would observe that, although the contagious nature of cholera in the ordinary acceptation of the term is generally denied, in more than one station the persons attacked were treated as if they were suffering from the most contagious disease, according to the usual meaning of the phrase. Parents have been prevented from attending their children, husbands their wives; the nearest relatives have been separated, and where the disease proved fatal have never seen each other again. Not to speak of the inhumanity of such a procedure, there can be no question that it is productive of much mischief, that it is calculated to engender the greatest alarm, and to cause a depression of spirits, which ought especially to be avoided. There is no evidence to show that there is any risk in attending on the sick, *when removed from the locality in which they were attacked.*"

Dr Cuningham, after discussing some suspected cases of contagion, sums up:—"Giving every weight to the circumstances narrated in each of these cases, it is still quite correct to state that in not one of the 100 bodies of troops, prisoners, and other communities which were under careful medical observation, and particulars regarding which are recorded in the notes, could the appearance of cholera be traced to importation."

Dr Cuningham goes on to show that the water theory has been most carefully examined, and found wanting. There is, he insists, no proof that the use of any particular water was attended with the appearance of cholera, and no proof that the disuse of any particular water was attended with the disappearance of cholera. The disease appears simultaneously over a great many districts at once; but it would be absurd to believe that the water of all these districts was contaminated at once.

"The similarity in the general history of cholera outbreaks is

quite inconsistent with the idea that the spread of the disease was due merely to a series of accidents; but leaving this point as one which can be easily tested by the details in each case, I would observe that there is no relation between the quantity of the so-called cholera material which is available and the results which it is supposed to produce. It will be conceded that the more abundant the material—the greater number of people, in fact, who are suffering from cholera, multiplying and eliminating from their systems this supposed poison—the greater must be the risk of contaminating the water-supply. But the outbreak never follows any such law. In Lahore city, for example, taking the number of deaths shown in the Registration Returns to represent half the number of attacks, 10 cases in June produce 86 in July, while 476 in August produce only 10 in September; and such is the history of the disease not in a few places only, but all over the country. In order to account for the results according to the water theory, the accidental pollution of the many sources of water-supply is supposed to occur at the commencement, just at the very time when it is of all others least likely to occur."

Dr Cuningham points out the hardships of quarantine and its uselessness, and ends by stating his confidence that general sanitary improvements have already done a great deal to save the people from the visitations of cholera. This he illustrates by showing out the diminished mortality from the malady in the jails in Upper India and Madras. In conclusion, he observes:—" But a still more specious argument which has been used is this, if non-contagion doctrines are accepted—If we are to believe that cholera is borne by the air or carried in some other mysterious unknown way, we are utterly helpless, we can do nothing, all progress is stopped. No argument could be more fallacious. It is the doctrine of contagion which has prevented progress. So long as men believe that all that is required to escape attack is to shun the sick, what interest can there be in sanitary reforms? The great lesson which the history of this epidemic and of all other epidemics teaches is this, that all we can do to diminish cholera is to carry out these reforms, and that these reforms will do much."

We advise all those who intend to form an independent opinion on the subject of the diffusion of cholera to make a careful study of these three works, especially of the Report of Dr Cuningham. In all three Reports the learned authors show that they have given great consideration to the subject; and, although they regard the question as a very serious one, their strictures show the courtesy and good-humour due to honourable opponents, all engaged in the endeavour to throw light upon a difficult and most important question.

Lectures on Madness in its Medical, Legal, and Social Aspects. By EDGAR SHEPPARD, M.D., M.R.C.P., Professor of Psychological Medicine in King's College, London, etc.

IT is an old custom for an author to appear humbly before the reader in a preface, and state his reasons for writing his book, and for the still more audacious step of publishing it. Sometimes, being doubtful whether these reasons will be accepted, he throws the blame on the importunity of friends, who, he fondly imagines, have perused the MS., and who insist that it shall not be lost to the world. Whatever may be thought of the sincerity of these excuses, they form an implied admission that there are quite enough of books in print already, and that some justification is required for adding to the heap. Dr Sheppard has no preface, but his justification seems to be in his first sentence. "This is a new chair, and I am a new professor." Professors are in the habit of writing text-books, and publishers look on their MSS. with a favourable eye, since a pretty sure sale may be expected from their students, especially if the professors be also examiners. Why, then, should Dr Sheppard not publish his "Lectures on Madness?" Taking into consideration the demands upon the time and attention of medical students, the Professor does not think he would be doing justice to them if he extended his course beyond seven or eight lectures. This, however, is totally insufficient to do justice to the subject. The whole work contains only 186 pages post octavo, and could be read in an evening, so it cannot stand in place of a text-book. Much of the matter treated of, or alluded to, is gone over in a superficial manner. The book, however, is pleasant reading to those interested in the subject; and where the author takes a little time, the description of the different forms of insanity are graphically done. The lectures, for example, on Puerperal Insanity and General Paralysis of the Insane contain judicious information in an agreeable form.

The author's style is striking and lively, but it would be well if he abstained from quotations and notes upon subjects not immediately connected with the matter in hand. The habit of quoting Shakespere is not an uncommon one with writers on insanity; but most people will be surprised to learn from Dr Sheppard, that the great poet was the first to shape the disease called General Paralysis "into a distinct and morbid entity;" and the reader has only to turn to Troilus and Cressida, act ii., scene iii., for an accurate and truthful description of the disease. It appears to us that the lines he quotes merely describe inordinate conceit and vanity in Achilles. One might as well say that, in Romeo and Juliet, Shakespere gives a description of nymphomania, or in Sir Andrew Aguecheek a portrait of cretinism. The following passages will give some idea of the author's style, and his opinion on two important points:—

"In all cases which are included under Dr Prichard's definition of moral insanity, it may be doubted whether, although there may be perfect freedom from delusions, there is not some disturbance of the intellectual faculties. Certainly, in those which he has recorded, and which have been analyzed and commented upon by various authors, the intelligence does not appear to have been altogether normal, and in the majority delusions were subsequently developed. In children, more frequently than in adults, you find this moral insanity dissociated from serious intellectual derangement.

"In cases of persistent suicidal and homicidal impulses, valuable lives are frequently preserved by the temporary adoption of what is termed 'instrumental coercion.' That treatment, I repeat, cannot in the largest sense be called humane which does not make the most effective provision against these aggressive outbreaks. It is wiser to be cautious against surprises than to be afterwards surprised into caution. One of the Commissioners recently told me of a case where their advice, that a determined suicide should be subjected to mechanical restraint, had been neglected at the cost of life. And I tell you—what a large experience justifies me in telling you—that you will be highly culpable if, through fear of official censure, you neglect to resort to this means of treatment when your judgment tells you that it is necessary. We shall never improve if we are not honest enough and bold enough to promulgate and sustain our true convictions."

A Manual of Public Health. Edited by ERNEST HART. London: Smith, Elder, and Co.: 1874.

Public Health—Part II. War in its Sanitary Aspects. London: Renshaw: 1874.

MR HART'S book has been published "for the use of local authorities, medical officers, and others," and we have no hesitation in saying that it is, in our opinion, the best manual on the subject. In these days the professional public is becoming more and more dissatisfied with secondhand information. For persons who have passed the grinding or cramming age, mere text-books are found comparatively useless, and information on any given subject is sought from some one who has made it his special study, and who is, in consequence, qualified and entitled to speak upon it with authority. The days of one-volume cyclopædias, the product of one mind, have passed away, and we now claim for many subjects in medicine and surgery, what Sir Robert Christison claimed years ago for medical jurisprudence:—

"No single individual can treat profoundly of the medico-legal relations of surgery, obstetrics, toxicology, insanity, and the general practice of physic. Nor shall I be content with the position of medical jurisprudence in this country, until three at least of their departments shall have been illustrated by an eminent surgeon, a scientific accoucheur, and some admitted authority in mental diseases, who shall each add to his own practical experience the study of the records of medico-legal literature, especially the fertile literature of Germany and France."

We congratulate Mr Hart on the happy idea of editing a work on public health, the leading departments of which have been illustrated by gentlemen thoroughly qualified for the task. A better selection could hardly have been made than Mr Michael, a barrister-at-law, to treat of the various sanitary acts; Professor Corfield, of University College, to discourse of the duties of the medical officer of health, in connexion with air, water, the removal of refuse liquid and solid, etc.; and, lastly, Mr Wanklyn, to give us an admirable digest of the present state of our knowledge as to the hygienic chemistry of water, air, and the more common adulterations.

In fact, the work consists of three distinct treatises by men practically acquainted with the subject on which they discourse, and hence the information given has that satisfactory ring about it—if the expression might be used—that gives the reader confidence in following it. The editor deserves credit for the general harmony that prevails. It was perhaps impossible to prevent altogether some overlapping, and we have seldom to note anything like contrariety of statement, such as we find as to the use of sulphurous acid as a disinfectant; Dr Corfield describing it, at p. 271, as "the cheapest, simplest, and most effectual;" and Mr Wanklyn, from the standpoint of the chemist, at p. 372, as "the most clumsy, costly, and ineffectual," going on to state that "all fumigation with chlorine and sulphurous acid are (*sic*) founded on a delusion, and ought to give place to more rational procedure." Notwithstanding this strong statement, we fully endorse, from some experience, what Dr Corfield claims for his procedure in disinfecting a room,—first using sulphurous acid, then thoroughly washing the walls with a solution of crude carbolic acid, and, lastly, free exposure to the air, if possible, for a fortnight. "Very frequently, however," adds the Professor, "the room has to be re-occupied on the same day. *Notwithstanding this, we have never known a single case in which fever has again broken out in a room which has been so disinfected.*" The italics are our own.

Dr Corfield, in his capacity as medical officer of health, and as visiting officer of the Local Government Board, has had much practical experience of the subjects of which he treats; and we have rarely, if ever, to differ from him, and then only on comparatively unimportant points.

He is thoroughly sound on water supply, which, he insists, should be pure. "It is ridiculous," he says, "to allow the water to be contaminated first, and then to seek to purify it by filters or other contrivances." And he gives no uncertain sound as to the amount per head of the population that should be supplied. "The least amount they should be allowed is, say, 20 gallons a day, and it is well to aim at a more copious supply, especially as the waste is almost always much greater than that allowed for in Professor Rankine's table, viz., 2 to 2½ gallons."

He is equally sound in his views on the disposal of the refuse of towns, and the various so-called conservancy plans. His conclusions we quote with special approval. "The medical officer may then advise the application of the dry-earth system in certain special cases. The first condition being, that the closets are placed outside the houses. It must always be remembered, in the first place, that frequent removal and constant supervision are absolutely necessary; and in the next, that even where the system is worked in the best manner possible, we are not sure that the compost is disinfected as well as deodorized. Indeed, the effect of dry earth is certainly a preservative one, and it has yet to be shown whether the poisons of special diseases are altered in any way or are merely preserved by being mixed with it. We know that seeds are kept and carried for long distances in dried earth; and so it is a mere presumption to assume that substances, which may or may not be of the nature of seeds, are destroyed by being mixed with it. It would be very interesting to know, if dry earth mixed with typhoid-fever stools is infectious or not, when it is dry. This we do not know. And it would also be interesting to know if such a mixture, after being kept dry for a certain time, would or would not become infectious when moistened. This we do not know. The presumption is, that it would. With such an uncertainty before us, the plain duty of those who have to advise on sanitary matters is, to oppose the introduction of this and all other conservancy systems with all their might, and not to hold out, as has been often done, delusive hopes of a pecuniary gain at the risk of damage to the health of the community at large." And again, "The manure obtained from drained middens or cesspits is of very little value, and scarcely ever pays the cost of collection. The single exception *being in the case of tubs or pails, which afford to our mind the best conservancy plan, both from a sanitary and economical point of view.*"

We agree with our author, that every plan that can be mentioned has its disadvantages; but we are pleased to find that that which has been successfully employed for several hundred years in the Old Town of Edinburgh, should still be looked on with favour by an Englishman.

Not the least valuable portion of the volume is the condensed but very valuable account of the analysis of water, air, and certain of the most commonly used articles of food, which are also unfortunately the most easily and the most commonly adulterated. Mr Wanklyn is an acknowledged authority in hygienic chemistry, and by his researches has enabled the analyst to speak with certainty on points which were formerly matters of surmise. To take the adulteration of milk, for example, the analyst can now not only speak with confidence to the impoverishment of the milk by the addition of water, but also to the equally important and more subtle adulteration, viz., the abstraction of the cream and the substitution of skimmed milk.

The second volume on our list is the concluding portion of a popular introduction to sanitary science by that sanitary veteran, Dr Guy. The former part, published in 1870, was a medical history of the English population, from the earliest times to the end of the eighteenth century. In the present edition, he treats of war in its sanitary aspects. The subject-matter was originally given in the form of lectures at King's College and at the College of Physicians. The historical method is adopted, and the treatment is highly interesting. Every medical officer of health, not to speak of practitioners generally, whether connected with the services or not, would do well to master the contents of this little volume.

Clinical Notes on the Electric Cautery in Uterine Surgery. By J. Byrne, M.D., etc. New York: W. Wood and Co.: 1873.

When Dr Byrne's observations on this subject appeared originally in the *American Journal of Obstetrics*, we were much pleased with them; and we think he has done right in reconsidering them and separately publishing them in this small volume.

The galvanic cautery is, we are sure, destined to a much more extended use than at present. In Edinburgh it has been occasionally employed for several years, but now it is attracting more general attention; and no doubt the improvement of instruments which is in progress, and in which Dr Byrne takes part, will greatly accelerate the diffusion of this valuable resource of surgery.

There is, however, another great want, namely, a good supply of the power to be used. All batteries are very bothersome, expensive, and disappointing. Attention to them is irksome to a practitioner. Now, electricians ought to be appealed to with a view to the supply of a convenient, cheap, and handy battery. For hospital purposes, or for special practice, we think we foresee great advantage in the magneto-electric machine, which has no cells or acids, and does not go out of order; but for private practitioners its bulk and expensiveness make it unsuitable.

Dr Byrne's instruments are good, but we could suggest improvements, especially in his improved loop instrument. His spiral cauterizer involves a good idea, and we recommend it to the notice of gynæcologists.

On Self-Culture, Intellectual, Physical, and Moral: A Vade-Mecum for Young Men and Students. By JOHN STUART BLACKIE, Professor of Greek in the University of Edinburgh. Edinburgh: Edmonston and Douglas: 1874.

THIS is an eminently wholesome and genial book, which old men as well as young will read with both profit and pleasure. As nothing dull or heavy could possibly emanate from our energetic and lively Professor of Greek, we need hardly say that midnight oil and a close chamber are never suggested by its fresh and vigorous sentences, but rather the breezy uplands and western seacoast views among which he often spends his autumnal recess.

But although lively, the book is high-toned, impressive, earnest, and altogether free from those occasional exhibitions of fun, bordering on eccentricity, in which the author sometimes indulges even in his class-room, and which ought only to be regarded as valves for the letting off the superabundant steam of a joyous and happy temperament.

Our space is limited, and it is difficult to make selections when all is excellent. One or two short passages from each of the three divisions of the book may serve as specimens. Speaking of the culture of the imagination, he says: "The life of Alexander the Great, of Martin Luther, of Gustavus Adolphus, or any of those notable characters on the great stage of the world, who incarnate the world which they create, is for this purpose of more educational value than the best novel that ever was written, or even the best poetry. Not all minds delight in poetry; but all minds are impressed and elevated by an imposing and a striking fact. To exercise the imagination on the lives of great and good men brings with it a double gain, for by this exercise we learn at a single stroke, and in the most effective way, both what was done and what ought to be done. But to train the imagination adequately, it is not enough that elevating pictures be made to float pleasantly before the fancy; from such mere passiveness of mental attitude no strength can grow. The student should formally call upon his imaginative faculty to take a firm grasp of the lovely shadows as they pass, and not be content till, closing the gray record, he can make the whole storied procession pass before him in due order, with appropriate badges, attitude, and expression. As there are persons who seem to walk through life with their eyes open, seeing nothing, so there are others who read through books, and perhaps even cram themselves with facts, without carrying away any living pictures of significant story which might arouse the fancy in an hour of leisure, or gird them with endurance in a moment of difficulty. Ask yourself, therefore, always, when you have read a chapter of any notable book, not what you saw printed on a gray page, but what you see pictured in the glowing gallery of your imagination. Have your fancy always vivid, and full of body and colour. Count yourself not to know a fact when you know that it took place, but then only when you see it as it did take place."

"Forwardness and pertness are a much more serious fault in a young speaker than a little nervous bashfulness. A public speaker should never wish to shake himself free from that feeling of responsibility which belongs to his position as one whose words are meant to influence, and ought to influence, the sentiments of all ranks of his fellow-beings; but that this feeling of reverential respect for the virtue of the spoken word may not degenerate into a morbid anxiety, and a pale concern for tame propriety, I would advise him not to think of himself at all, but to go to the pulpit or platform with a thorough command of his subject, with an earnest desire to do some good by his talk, and to trust to God for the utterance."

"The best preservatives against the crampling force of merely professional study are to be found in the healthy influences of society, in travel, and in cultivating a familiarity with the great writers—special poets and historians—whose purely human thoughts 'make rich the blood of the world,' and enlarge the platform of sympathetic intelligence."

On Physical Culture the Professor writes very sensibly, and, as might be expected, quite *con amore*:—

"Every young student ought to make a sacred resolution to move about in the open air at least two hours every day."

"In an age when the whole world is more or less locomotive, the student who stays at home, and learns in a gray way only from books, in addition to the prospect of dragging through life with enfeebled health, and dropping into a premature grave, must make up his mind to be looked on by all well-conditioned persons as a weakling and an oddity."

"The music of well-regulated emotions imparts its harmony to the strings of the physical machine, and freedom from the blind plunges of wilfulness keeps the heart free from those fierce and irregular beatings which wear out its vitality prematurely. Therefore, if you would be healthy, be good; and if you would be good, be wise; and if you would be wise, be devout and reverent, for the fear of God is the beginning of wisdom."

The subject of Moral Culture is treated very admirably, and is perhaps the best part of the book. Had we space, we would quote largely from it, but a few short, isolated sentences must suffice as specimens of the author's train of thought and style:—

"I presume there are few more virtuous men than some who talk of laws of nature, invariable sequence, natural selection, favourable conditions, happy combination of external circumstances, and other such reasonless phrases as may seem to explain the frame of the universe apart from mind. But to a healthy human feeling there must be always something very inadequate—say rather something abnormal and monstrous—in this phasis of morality. It is as if a good citizen in a monarchy were to pay all the taxes conscientiously, serve his time in the army, and fight the battles of his country bravely, but refuse to take off his hat to the Queen when she passed."

"Primroses grow only in the spring; and certain virtues, if they do not put forth vigorous shoots in youth, are not likely to show any luxuriant leafage in after age."

"The best preventive against idleness is to start with the deep-seated conviction of the earnestness of life."

"The octogenarian poet-philosopher Goethe, when sinking into the darkness of death, called out with his last breath, 'More Light!' What every young man should call out daily, if he wishes to save himself from the narrowing crust of professional and other limitations, is, More Love!"

"A good man will as much as possible strive to be shaken out of himself, and learn to study the excellences of persons and parties to whom he is naturally opposed."

We very cordially recommend this little book (for it is comprised in 91 pages) to all our readers, but specially to young practitioners and medical students. It is full of good sense and good feeling, creditable at once to the head and the heart of its able and genial author.

Lessons in Laryngoscopy, including Rhinoscopy. By Dr PROSSER JAMES. London: Baillière, Tindall, and Cox.

THE style of this little work is not unlike that of some others bearing on the same subject. It may be divided into several sections. 1. Treating generally of the instruments used for viewing the laryngeal cavity, and the method of employing them. 2. Description of what is seen in the natural condition of the parts. 3. A history of laryngoscopy, including Garcia's paper, read before the Royal Society of London in 1855, in full. 4. A short description of the morbid appearances presented in the larynx and adjoining parts. And, 5. Some remarks upon treatment, with a description of various instruments necessary for carrying that out.

We would specially draw attention to the part which deals with Rhinoscopy, as being good, but think the paper of Garcia might have been dispensed with without detracting from the usefulness of the book. We can recommend the work particularly to those beginning the study of this subject, as in it they will find simple explanations of things often made more difficult than need be to the reader, and simply from the use of too many words.

There are some very nice illustrations, although not all quite new, scattered through the pages of the book.

Traité Pratique d'Auscultation appliquée au Diagnostic des Maladies des Organes Respiratoires. Par le Dr L. MAILLOT. Paris: J. B. Baillière et Fils: 1874. Pp. 542.

OVER 126 pages of this work are occupied by an interesting discussion as to the various causes of cough; the rest is devoted to the auscultatory phenomena of the lungs in health and disease. Dr Maillot is a well-known man, who thirty years ago published, along with Piorry, a work on the plessimetric examination of the kidneys. Belonging to Piorry's school, it is no wonder that he knows no stethoscope since that of Dr Williams, which he regards as the newest. Camman's binaural stethoscope and Scott Alison's double one are both alike ignored, though both of them are thought to have special advantages in diagnosis. Laennec, Beau, and Skoda are the three authors whose views, in regard to the production of auscultatory phenomena, he expounds, and seeks to reconcile by a process of ratiocination. But an appeal to reason will not explain differences in the interpretation of physical phenomena which are so radically diverse as those of Laennec and Skoda. The latter appeals to experiment and to the laws of sound, and by these he must be judged; and we are much mistaken if these laws do not bear out all the statements he has made, though they may also go a little further and explain a little more clearly than even he has done. For the rest, Maillot's book is neither better nor worse than other books of the same character and size.

Chirurgisches von der Weltanssteltung. Von Dr JOS. ENGLISCH, Docent für Chirurgie an der Wiener Universität. Wien Med. Presse: 1873.

THE author of this paper begins by stating that, while in certain diseases the remedial agents are becoming ever simpler and less numerous, orthopædic instruments and instruments for diseases of the genito-urinary organs have undergone a remarkable multiplication, it being a difficult matter to use the same instrument with a like result in these diseases, from their great variety of symptoms.

The first apparatus described is an American method of treating fractures, the principle of which is the substitution of imaginary lines passing through the axis of movement of a limb by flexible metal bands. Such lines in case of fracture may be bent, lengthened, or shortened by displacement, and in accordance with the direction of such displacement. The metal bands can neither be lengthened nor shortened, and are rough that they may be held in position by bandages. The following is the succession of dressings:—(1.) The limb is wrapped in cotton wool; (2.) thereon, a layer of plaster-of-

Paris bandage; (3.) the metal bands are placed in position, and retained by a superficial layer of plaster-of-Paris bandage. At the knee-joint, the situations of these bands would be in the long axis of the limb, and corresponding to the internal or external condyle, according to the direction of the fracture and its tendency to displacement, and another midway between the first and the centre of the patella. The correctness of the principle is admitted, while the claim of the method to originality is questioned, inasmuch as bands of pasteboard have been used in like manner, at the points indicated, to give support to the isinglass dressing once used. Its lightness is said to be its chief recommendation. With a passing allusion to Bonnet's wire-gauze apparatus used in transport and for swinging limbs, the rest of the paper is taken up with a description of the various means employed in giving support in spinal curvature. Danish and German mechanists seem to have been the chief exhibitors in this department, though other countries are also represented. The author characterizes the French instruments as models, and, from the delicacy of their workmanship, evidently intended for exhibiting purposes—a fault, doubtless, if they be too frail, but one much less frequently committed than that of manufacturing what may be called harness, too ponderous in many instances even for strong men. The paper closes with an enumeration of the necessaries for a useful apparatus, and gives in detail the measurements to be taken, when providing a patient with such a support is contemplated.

Galvano-Therapeutics. A Report made to the Illinois State Medical Society, 1873. Philadelphia: Lindsay and Blakiston.

THIS volume, apparently written by a Dr Prince, is a specimen of that mass of useless literature which has arisen in consequence of the growing importance of the subject and the imperfection of our knowledge. Such reports, though evanescent, might be extremely useful. A résumé of the progress made in galvano-therapeutics, a clear explanation of the best forms of apparatus and the mode of using them, would aid the student in his investigations, the practitioner in his practice. In this instance the writer exhibits a certain practical acquaintance with some of the necessary implements, and possibly such was his sole desire. He fails utterly in his attempts to describe them for the benefit of others. In the clinical portion of his work he is still more unhappy. It is a record of cases of his own, meant to illustrate the varied uses of galvanism. In these circumstances, one expects to find that the examples under each head are typical and complete. The disappointment is grievous. In some cases the diagnosis is incomplete, in others plainly erroneous; in many the conclusions drawn are completely vitiated by the fact, that other and presumably or plainly more efficacious remedies were simultaneously employed. His sins of commission are thus flagrant, those of omission are not less so. Many of the most important medical applications are not mentioned; "osteo-encephaloid" cancer, elephantiasis, and stricture, are the singular illustrations of electrolysis which he selects; and amputation of the cervix uteri is the only application of the galvanic cautery.

The book is full of vague hypotheses, hazy descriptions, and slipshod grammar. Its style may be inferred from the following sentence, in which the author describes the effect produced by galvanism on "Miss N., aged 19," a young lady who suffered from "general prostration." "With the grist well ground out below, and the hopper filling above, the working of the mill became all right."

Natural History of North China. Compiled chiefly from the Travels of Père ARMAND DAVID. Translated by GEORGE SHEARER, M.D. Edin., North China.

SCIENCE has not always been on the best of terms with the Roman Catholic clergy; but in every age there have been eminent men connected with that body who have done good service to her cause, even when their primary aims were of an altogether different nature. In geography especially, from the days of Rubruquis and Carpini to those of Huc and Gabet, her intrepid missionaries have often raised the veil concealing regions that were hitherto all but unknown. Father Armand David, the chief hero of the little work before us, is a Lazarist friar, but of a more modern type, for in him the love of natural history took the first place instead of a secondary position, and his ecclesiastical superiors in the Chinese mission, to which he was attached, wisely allowed, and even encouraged, him to follow out the path to which he was naturally inclined. He first, when stationed at Pekin, worked steadily at the botany and zoology of Northern China, carrying his explorations from the plains of Pe-che-li into the mountains that shut out Mongolia, over which the Tartars came to subjugate the empire. Subsequently, he ascended the great river Yang-tse-kiang, and passed into the far west, till he had crossed the boundaries of China. There he resided for some time at Moupin, in the eastern province of Thibet, accumulating specimens of natural history in a region of snowy peaks, forming the eastern edge of the great plateau. Ultimately his health gave way, and he was obliged to return home to France, preceded by his collection, which reached Paris before the great siege. He himself only came in time to find the Prussian lines between him and the fruit of his labours, which, it is to be hoped, received no injury at the hand of foe or countryman.

The little work, in pamphlet form, before us, though mainly aken up with the labours of Father David in the north and west of China, also summarizes the results of the work of British naturalists—Fortune, Swinhoe, and others—in the south-east and south of the empire. It is therefore a meritorious attempt to place before the scientific world the natural history of a vast region, whose southern confines are inhabited by the paroquets of the tropics, and from whose northern mountains the blue arctic fox steals down for its winter support; truly a world in itself.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING V.

Wednesday, 4th March.—Dr RUTHERFORD HALDANE, *President, in the Chair.*

I. *Dr Warburton Begbie,* by permission of the Society, read his paper before the exhibition of pathological specimens, as he was unavoidably compelled to leave the meeting at an early hour. Before commencing, he expressed the high satisfaction he felt in addressing one of his oldest and best friends in the presidential chair of the Society. He then read his paper, entitled ALBUMINURIA IN CASES OF VASCULAR BRONCHOCELE AND EXOPHTHALMOS. The paper is printed in full at page 880 of this number of the Journal.

As Dr Begbie had to leave the meeting at once, no discussion followed the reading of the paper, but the President in a few words alluded to its interesting nature.

II. *Dr George W. Balfour* exhibited an example of SIMPLE ULCER OF THE DUODENUM, situate within a couple of inches of the pylorus. He stated that the patient from whom this preparation was taken had been admitted into Ward V. of the Royal Infirmary on the preceding Thursday, complaining of constantly recurring pain in the scrobiculus cordis. He was 19 years of age, stunted in his growth, and emaciated. He had lost all his teeth from caries, and evidently laboured under serious gastric derangement, evinced mainly by the recurring pain and his defective nutrition, for which no substantive cause could be discovered. On the Saturday following his admission he was up, going about the ward, but with a depressed, anxious expression, complaining of pain, sickness, and vomiting, and with some degree of tympanitis. On palpation the pain and uneasiness were not increased. He died somewhat suddenly during the night. The vomited matter consisted of a small amount of half-digested farinaceous food, floating in a large quantity of dark-coloured fluid, which was unfortunately thrown out before being tested for blood. After death the abdomen continued tympanitic, but not greatly so; upon opening it, a quantity of gas escaped. The peritoneum was not injected; there was neither serum nor any foreign matter in it. The stomach was slightly congested, and about two inches from its pyloric extremity the ulcer exhibited was found adherent to the pancreas by its cartilaginous base, and communicating by a small opening with the cavity of the peritoneum. It was very evident that in this case peritonitis had no part in causing the death; that in its essence it was a simple case of acute indigestion, proving fatal by reflex action on the heart—a case of what often went under the name of *syncope senilis*, the age in this case being relative and not actual, and produced by previous debility and malnutrition, the result of the duodenal ulcer. It was entirely analogous to those cases in the horse and cow which went under the name of stomach staggers (the *Vertige abdominale*, or *Indigestion vertigineuse* of the French veterinarians). The older members of the Society would take a greater interest in this case from its similarity to a most interesting case brought before it about thirteen years ago by Mr Benjamin Bell, at that time the President. That case presented many features of even greater interest than this one, to which it was, however, entirely analogous. The present President, Dr Haldane, had doubted the occurrence of perforation during life in Mr Bell's case, and this one in so far confirmed this view, as that in it death had occurred under somewhat similar symptoms without a trace or a suspicion of the pre-occurrence of peritonitis—a fact which was also not rare in those other animals of the mammalian class already referred to.[1]

III. *Dr Joseph Bell* showed—1. THE BONES OF THE ANKLE-JOINT AND TARSUS, which he had recently removed by Syme's amputation from a boy æt. 12. The specimen was interesting from the fact that about a year ago he had *excised* the ankle-joint for disease of the joint and of the lower end of the tibia, by the subperiosteal method. Bone was very extensively reproduced, and the patient made a good recovery from the operation, but unfortunately disease again recurred in the tarsus. The lower end of the tibia was quite as large as the other one, though it had been entirely removed. 2. THE ENTIRE EAR, which he had excised on account of epithelial cancer from an old farmer, æt. 70, who had suffered from the disease for more than a year. 3. Two good specimens of RAPIDLY-GROWING ENCHONDROMATA FROM THE BONES OF THE HAND. He showed by sections how the tumours were not only growing from he medullary canal, but also were continuous with the cartilage of he epiphysis, and thus that any partial operation was unjustifiable; and he contrasted them in this respect with exostosis, which in many cases can be removed by section of its neck, without necessarily removing the bone from which it grows.

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[1] *Vide* Mr Bell's case, at p. 783 of the number of this Journal for March 1861, and the very interesting discussion upon it at p. 840 of the same Journal.

IV. *Dr Cadell* exhibited a HORN removed from a female, aged 48. It was situated on the hairy scalp, had taken seven years to grow, and was one inch and a half in length.

V. *Dr Argyll Robertson* then read a paper on the operation of REPOSITIO CILIORUM FOR TRICHIASIS, which will appear in a subsequent number of this Journal. He demonstrated the mode of operating by diagrams on the black-board.

Dr Joseph Bell observed that this paper, like all Dr Robertson's contributions to ophthalmology, was equally brief, precise, and clear. He believed that in suitable cases—*i.e.*, in those where the misplaced ciliæ are few, large, and visible—this operation may be of great value; but he feared that in those more numerous cases in which the offending eyelashes are numerous, small, and semi-transparent, this method of operating will be of little use, and the surgeon will have to fall back on operations like that of Mr Streatfeild.

VI. *Dr Chiene* read a paper entitled, NOTE OF A CASE OF FRACTURE OF THE RADIUS, which will appear in an early number of this Journal.

Dr Joseph Bell remarked on the value of the paper as a recent dissection of a recent case of the fracture known as Colles's fracture, though in it perhaps the comminution was greater than usual, especially into the wrist-joint. He preferred, in describing these cases, the use of the words palmar and dorsal rather than anterior and posterior. He believed much more in the direction of fall, and the manner of impact of the body, as the cause of the displacement in this fracture, than in the action of the supinator longus. Dr Chiene thought that the complexity in treatment and various means adopted by different surgeons was caused by a want of knowledge as to the pathology of the fracture; he found explanation (1), in the smallness of the distal portion of bone fractured, rendering the application of splint to it difficult; (2), in the proximity of the fracture to a complicated articulation, and to numerous tendons; (3), to the advanced age of many of the patients who met with this fracture. He was more afraid of stiffness of the fingers as a result than of non-union, so in private practice, with intelligent patients, was in the habit of using Gordon's splint, from which he had obtained excellent results. In hospital he generally used palmar and dorsal splints, but not so long as to interfere with the movement of the fingers, by which their stiffening could be prevented.

Dr Miller remarked that he had successfully employed the long dorsal and short palmar splints in Colles's fracture. He thought the palmar splint might be brought farther down than Dr Chiene recommended. Though on the skeleton there is a projection at the lower end of the radius, on the living body there is almost a straight line down to the ball of the thumb. Dr Miller would therefore carry the palmar splint as far down as this latter projection, and so get a better control over the fragments and over the motion of the wrist-joint. Dr Miller could not believe that the amount of comminution in Dr Chiene's case could occur in the cases of Colles's fracture generally met with, as then there would certainly be grave inflammatory implication of the wrist-joint, which complication, according to his experience, was never met with except in very exceptional and severe cases, such as he believed this one related by Dr Chiene. Dr Miller thought the paper a very interesting and valuable contribution to surgical pathology, and suggested to Dr Chiene to carry out his observations on the mechanism of the cause of this fracture. Dr Miller stated that he did not agree with Dr Bell in his praise of Dr Gordon's splint. He thought it permitted too free motion of the arm, and was useful only when the patient could be kept in bed with the arm on a pillow.

Dr Taylor congratulated Dr Chiene on his good fortune in obtaining for dissection a recent case of Colles's fracture. Our knowledge concerning the deformities which result from this accident, was mostly derived from the examination of dried bones found in cabinet collections, and those, too frequently, had no history, so that we did not know which mode of treatment had been adopted in the various cases, nor, indeed, whether any surgical treatment whatever had been used. There can be little doubt, that often the fracture unites without treatment, and that often, when treated, the treatment is detrimental, the bandages being applied in such a way as to press unequally, and to favour the diminution of the interosseous space at the site of fracture. We frequently hear of bone-setters successfully treating dislocations of the wrist. Now, we know this is one of the rarest injuries to which the human frame is liable, and we may safely assume that these so-called dislocations are simply cases of Colles's fracture, which, indeed, often look very like dislocations. The fact that these cases do well under such treatment, and that in the hands of surgeons, whose treatment of them is so varied, they do well, is sufficient proof that the difficulties besetting the subject are not insurmountable. But if any additional evidence were needed, it would be found in the fact, that the fracture unites at all under the use of Gordon's splint, for anything more opposed to such a result could hardly be devised. Laying aside the theoretical grounds upon which this splint is recommended, for the simple reason that they are purely theoretical, and looking at the thing in its practical bearings, you find that you ve to apply to an injured member, two heavy, cumbersome pieces

of wood. These are padded and applied to the arm. They are kept in position by two leather straps, or rather, they are not kept in position, for soon after being applied, the splints begin to revolve either from the friction of their sharp angularities against the person and bedclothes, or by the apparatus as a whole having some mysterious sympathy with the solar system—it matters not which. No man ever has seen or shall see these splints in the same position at the end of four-and-twenty hours as they were when he applied them. They will have performed in that time at least a quarter of a revolution. And where is the fracture? Probably worse than it would have been if left to the bonesetter aforenamed, or to nature alone and unembarrassed. If, as Dr Miller has said, we could get things to remain exactly in the position we place them, such an apparatus might be of use; but, unfortunately, our patients have to sleep as well as wake, and to move voluntarily and involuntarily, and these contingencies are not provided for by Dr Gordon's splint. Dr Chiene has said, with a degree of sense which is only equalled by his good surgery and pathology, that union at the site of fracture would be favoured by the application of rectangular splints, to restrain the movements of the elbow-joint. This seemed an excellent suggestion, for often indirect causes led to non-union. In one case of fracture of the femur, treatment had to be prolonged for months on account of the patient having been in the habit of playing on the concertina and moving it in the air—in order to produce the effect of the swell—while his leg was in the long splint. The movement produced in the injured femur by such means must have been very slight, still it was sufficient to produce non-union and its attendant discomforts. If this is the case, and if movements at the elbow-joint are likely to interfere injuriously with the treatment of Collis's fracture, how much more will movements of the hand and wrist produce this effect, where the connexion is so much more immediate, and the leverage so much more direct! Now, when Gordon's splint is employed, the hand is allowed to hang loosely, and the patient is encouraged to use it. The indications of treatment in this and every other fracture are, first, to restore the bones into apposition, and then to adjust our apparatus so as to meet and obviate the tendency to displacement. This will vary according to the nature of the accident, and the direction of the force which produced it. If we may take the cast exhibited by Dr Chiene as a fair example of the injury—and I believe it to be so—then the hand is displaced backwards and towards the radial side. The old-fashioned treatment therefore was not so far wrong, for under it the forearm was placed between pronation and supination, the hand inclined towards the ulnar side, and kept in that position by obliquely-shaped palmar and dorsal splints, so that the tendencies to displacement were met and averted in either direction, while the arm and hand were retained at perfect rest.

Dr Chiene in reply explained, that while knowing well Gordon's splint, he had not referred to it intentionally, as he had not yet been able to obtain Dr Gordon's own account of it, notwithstanding that he had written to Dr Gordon for it. He then gave an interesting account of the various methods adopted by the surgeons in the Edinburgh Infirmary, in treating this fracture, proving that the majority of them used various modifications of the long anterior and posterior splints.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING VII.

Wednesday, 11th February.—Dr J. MATTHEWS DUNCAN, President, in the Chair.

Dr Carmichael read a paper on the ANTISEPTIC TREATMENT OF MAMMARY ABSCESS, and read Notes of a Case illustrating the benefit to be derived from it.

Dr Pattison said he had found the antiseptic treatment of great service in boils; indeed, the good effects were marvellous. He had not tried it in abscess of the mamma. He called attention also to the good done in such cases by the compound tincture of benzoin, where, by healing the nipples, it frequently prevented abscess.

Dr Gordon said that "prevention was better than cure;" and he was in the habit of devoting considerable attention to the nipples. Oleaginous substances, he thought, were better for the nipple than astringents. When, unfortunately, abscesses did form, he recommended the breaking down of the intervening septa, and the throwing of them into one. He thought our most eminent surgeons had come to the conclusion that the antiseptic method prevented putrefaction, but not suppuration.

Dr Sanderson thought Dr Carmichael's plan a good one; but, in the country, as he found, it was very difficult to get patients to follow out intelligently any plan one laid down.

Dr Ritchie was much interested in Dr Carmichael's paper, and thought the case reported a peculiarly happy one. Dr Ritchie did not approve of breaking down the septa, as it was apt to injure the mamma much, and prevent future lactation. He had opened as many as six abscesses in a breast, and yet, subsequently, this did not interfere with nursing. Dr Ritchie's plan was to open the individual abscesses, and apply water dressing. He did not like poultices. He could not approve of the antiseptic plan, unless it might be in the case of a very large abscess. He laid much stress on constitutional treatment, and he had much confidence in tonics, such as quinine and iron. He repeated, that a distinction should be drawn betwixt several small and one large abscess, in order to guide local treatment.

Professor Simpson said a distinction was requisite as regards the treatment of the different kinds of abscess. In the deep glandular variety, where there might be various abscesses forming, he agreed with Dr Gordon that it was desirable to throw these into one. Some years ago he had tried the antiseptic method in the glandular variety, but he found that it did not shorten mischief, others of the glands having become distended. The antiseptic treatment might be a useful corrective where there were numerous abscesses; but he had seen a big cavity collapse and heal without this treatment. He believed that, by a full evacuation of the cysts, you got a more rapid cure.

Dr James Young said he had had quite a run of mammary abscesses of late, and some of his cases had been very troublesome. In one he had to open four or five times, yet there was fresh suppuration. He approved of Dr Gordon's suggestion; he had carried it out with much benefit; and he also approved highly of Dr Ritchie's constitutional treatment. He could not agree with Dr Carmichael, that eczema was more liable to be produced on the skin of the mamma than on any other part of the body. The breast was a gland very liable to suppuration, and his experience was, that when this happened three or four times, the mamma was of little future service.

Dr Keiller was sorry he had not heard Dr Carmichael's paper. He fully approved of the carbolic treatment, and used lint dipped in carbolic oil. These abscesses should be opened in a way fitted to exclude air—say, through carbolized lint; and thus the extension of suppuration would be averted. He should like to say a word or two on the value of compression by means of adhesive plaster. This was an excellent preventive. When he had to open a mammary abscess, he usually gave chloroform, as there was often intense pain in searching for matter. After opening, the breast should be well squeezed, so as to get a full evacuation. It was of great importance to prepare the nipples, and he was convinced of the great superiority of oleaginous preparations over astringents for this purpose.

Dr Ritchie inquired of Dr Carmichael if, in his case, fever showed itself on the third day? Dr Ritchie had never seen fever so early. He might state, further, that he believed in applying the child early to the breast.

Dr Carmichael said the child was applied early, but that it was an indifferent sucker.

Dr Duncan said that he had no experience of the antiseptic treatment of mammary abscesses. His observation of such cases had extended over a period of nearly thirty years, and he must say that he had witnessed great improvement during that time. When he was a student, the practice was to seize the earliest possible moment for opening; the notion of waiting was never thought of. In whitlows it was necessary; but formerly all suppurations were opened too soon. In regard to the mamma, there had been two evils:—*1st*, Women had had their breasts incised when there was no pus to be found; *2d*, The premature opening was a prolific cause of a succession of abscesses. It was clearly established that it was bad practice to open early; we must see the thing matured if we wish for a speedier cure. The treatment of mammary sinuses was capable of much improvement; and he hoped for some good results by means of the plan recommended by Dr Carmichael. He was sure that the antiseptic method was very valuable in a great variety of circumstances and cases.

Dr Carmichael replied. He said his paper was somewhat fragmentary, and intended chiefly to provoke discussion. In reference to Dr Gordon's practice of breaking up the septa, he (Dr Carmichael) did so also, but with care and gentleness, and chiefly for exploratory purposes. He was chary of injuring the breast. He agreed with Dr Ritchie as to the value of constitutional remedies.

SESSION XXXIII.—MEETING VIII.

Wednesday, 25th February.—Dr J. MATTHEWS DUNCAN, President, in the Chair.

I. *Dr Angus Macdonald* read a paper on a case of TWIN PREGNANCY, WITH DOUBLE PLACENTA PRÆVIA (partial), and with both Fœtuses presenting Transversely, with Remarks. (This paper will appear in a subsequent number of this Journal.)

Dr Duncan inquired, as a preliminary to the discussion, whether Dr Macdonald could give a description, however brief, of the condition of the placentæ after birth.

Dr Macdonald said that, having had to go to another case, he had neglected to examine them.

Dr M'Rae said Dr Macdonald's paper was interesting and learned, but he thought he had digressed rather in introducing the subject of zymotic diseases. His not having given a description of the condition of the placentæ was also a great mistake, and robbed the paper of its essence, as it were.

Dr Wilson said, with reference to the question of zymotic disease, he thought pregnant women quite as liable to fevers as non-pregnant. He rather thought that during pregnancy, too, fevers were more fatal.

Dr Connel said he had met with a case somewhat similar to Dr Macdonald's. The first child, a very small one, he brought home by means of the forceps, the head presenting; the second, almost immediately after, by the feet. He believed this woman to have been saved by the rapidity of the delivery, for hæmorrhage was great. However unable to account for the tendency to abortion in such cases, it was a beautiful provision that such a thing happened.

Dr Ritchie had seen one of the cases mentioned by Dr Macdonald. Her condition when she aborted was somewhat doubtful; in fact, she was not sure that she was pregnant until abortion took place. He rather thought the temperament of the woman might have helped to induce abortion. In other two cases he had seen the women make a good recovery after aborting, though one of them had a severe rigor the second day.

Dr Carmichael had seen five cases of pregnancy complicated with smallpox. They all aborted, some at the earlier, others at the later months. He had seen two cases of scarlatina where abortion was also occasioned. While in the Infirmary, under Dr Sanders, he had observed that typhus rarely, if ever, caused abortion, while typhoid did.

Dr Dickson had a smallpox case at the sixth month. The woman went to the full time, but the child resisted vaccination.

Dr Underhill said that his personal observations had led him to support the view which he observed Schroeder held, viz., that typhus was rare, and typhoid the most common. He had had one case of typhus. It seemed that it was not so rare as Dr Macdonald thought, to have placenta prævia with twins. Dr Connel had mentioned one case, and he himself had met with one. The question as regards the cause of placenta prævia was an interesting one; he thought it might be due to pre-existing disease of the mucous membrane—at least this might be a predisposing cause.

Dr Bruce also remembered meeting with one case of twins complicated with placenta prævia. Delivery occurred at the seventh month.

Dr Bell had not heard all Dr Macdonald's paper, but thought that portion of it which he had heard interesting. He was of opinion that in Dr Macdonald's case the placentæ were attached above what was called by Barnes, incorrectly as he thought, the dangerous zone. It was deeply to be regretted that Dr Macdonald did not examine the mass, where it adhered and where it was not adherent. He was convinced that Dr Macdonald's was a partial case, and that a lobe or two of the placenta merely had come down. Barnes's description was much exaggerated, for it was impossible that the placenta could become adherent to the cervix, though a lobe might gravitate below the safe locality.

Dr Duncan had no facts to present as a contribution to the question of the liability or not of pregnant women to fever or abortion, or as regarded the mortality in zymotic cases. Dr Macdonald had expended some able criticism in his paper, but he (Dr Duncan) would not at present discuss the questions raised, nor yet reply to those objections which had been suggested with reference to his own views, and which he had but recently published. At a future time he might do so.

Dr Wilson inquired if Dr Macdonald thought hydramnios a frequent cause of hæmorrhage.

Dr Macdonald replied. He said, referring to Dr M'Rae's remark about his having digressed somewhat, that it was quite relevant in his remarks to refer to the important question of zymotic disease; and though he had done this, he did not think that it should have withdrawn the interest of the Fellows from the subject of placenta prævia. The question was, in fact, suggested by some points in one of the cases, and, if he needed an excuse, it was to be found in the fact that there seemed to be no little doubt, among practitioners especially, as to the fatal or non-fatal character of fevers during pregnancy. For instance, it had been said by an eminent consultant that typhoid happening during pregnancy was likely to go on all right; but if abortion took place, the case was likely to prove fatal. As regards the cause of placenta prævia, he (Dr Macdonald) thought the large size of the uterine cavity was a more probable cause than any disease of the mucous membrane. He agreed with Dr Bell that the placenta could not be implanted in the cervix; were it so, fatal results would often happen, the cervix being in a condition of paralysis. In reference to Dr Wilson's question, he might state that he did not think hydramnios a frequent cause of hæmorrhage.

II. *Dr Duncan* read a paper on the SIZE OF APERTURE NECESSARY FOR THE PASSAGE OF THE PLACENTA AND FOR THE PASSAGE OF THE ACCOUCHEUR'S HAND. (This paper appears at p. 890 of this Journal.)

Dr Charles Bell thought Dr Duncan's paper interesting, and the subject an important one. He had sometimes found it easier to get the hand in than out in malformed pelves. His left hand was smaller than his right, but he required an aperture of from 2½ to 3 inches. The hand will enter more readily if the opening be elliptical. Dr Bell illustrated this by several cards he had made with such openings in them.

Professor Simpson said the shape of the opening was of great importance. The comparison betwixt a circular opening in cardboard and the dilatable os, which could be made to yield transversely and otherwise, was not a proper one; so Dr Bell's elliptical opening was required to complete the observation. In connexion with this he must still express his opinion that if the os were dilated sufficiently to enable the hand to enter and separate the placenta, the latter could be removed without injury to the os.

Dr Dickson said his hand could go through a circular opening of 2½ inches.

Dr Connel said that the cardboard proved little, as it was unyielding; in fact, the comparison of the resistant board with the dilatable os was a mere fiction.

Part Fourth.

PERISCOPE.

Abstract of a Pamphlet on the Origin of Syphilis, by Dr Edmond Bassereau.—The first chapter of this thesis contains the evidences of venereal affections, from writings prior to the year 1494. The second chapter treats of these evidences after that date.

M. Bassereau directs attention to Leviticus, chap. xv., from which it is seen that, at that early date, the true character of gonorrhœa was well known. With reference to the diseases and pains of Job and David, our author thinks that those who bring forward these facts as proof of the ancient existence of syphilis, do so on pure hypothesis.

The Greek and Latin writers speak of gonorrhœa, orchitis, stricture of the urethra, vegetations on the genitals, and, lastly, of chancres. But they are silent as to the cause of these affections, and none of them speak, as is done in Leviticus, of the essentially contagious character of urethral discharges.

Still, from the many allusions made by poetical satirists, there can be little doubt that the contagious nature of these diseases was well known. Martial says:—

> "Ficosa est uxor, ficosus et ipse maritus
> Filia ficosa est, et gener, atque nepos."

On passing to the medical literature of the middle ages, we find descriptions of vegetations, fissures, and condyloma. William of Salicit, in the year 1270, says, "White or red pustules, miliary vesicles, chaps and corruptions, and similar things, which form on the penis or prepuce, result from connexion with an unclean woman, with a courtezan, or some such cause."

Furthermore, the relationship between inguinal buboes and ulcers of the penis, is clearly noted. Lanfranc of Milan, Bernard Gordon, and others, speak in analogous terms of ulcers of the penis, which they considered purely local affections.

Leprosy was confounded with syphilis, and was supposed to be transmissible by coitus. Daniellssen and Boeck have shown that leprosy is not contagious, but is almost always hereditary. Basin has observed the coexistence of syphilis among the leprous.

M. Bassereau observes, that the buboes, so often mentioned by the fathers of medicine, are more commonly complications of the simple chancre than of the infecting one. Taking the results of the researches on this subject by MM. Bassereau, Fournier, Belhomme and Martin, Millet, we find that out of 486 simple chancres, not followed by syphilis, there occurred 166 suppurating buboes. On the other hand, 520 infecting chancres followed by syphilis, only gave rise to 20 cases of inguinal suppuration.

The phagedenic character of chancres, described in ancient times and in the middle ages, is no more a proof of the ancient existence of syphilis. For among 51 cases of phagedenic chancre, observed by MM. Bassereau, 40 cases occurred with simple and 11 only with infecting sores.

The second chapter treats of the evidences of venereal disease found in writings subsequent to 1494.

M. Bassereau states, that nothing has been written by physicians prior to the end of the fifteenth century to indicate their acquaintance with syphilis; and that what is said of venereal disease has only to do with simple chancres, buboes, gonorrhœa, or their complications.

Marcellus Cumanus and others, who wrote of syphilis between the years of 1495 and 1500, are unanimous in calling it a new and epidemic disease, which, perhaps, might have existed at a remote period, but had never been seen in their time. Till the year 1550, all physicians are supporters of the doctrine of the new appearance of syphilis. During this period of fifty-six years, we find only one supporter of the ancient origin of the disease,—he is Sebastian Montuus, in 1537. He considers syphilis of ancient origin, and that the causes producing it had existed from all time. Montesaurus and John Paschalis, on the other hand, held that the disease did not exist at all, but that a combination of diseases had been considered as one.

The dates assigned by contemporaneous authors for the first appearance of syphilis in Europe, vary a little. Gilinus says it commenced in 1496; Vigo gives the time of its origin in Italy in December 1494.

Physicians at this time believed the disease to have developed itself spontaneously under the influence of the great floods which occurred in 1495, or by some special visitation of providence, or the conjunction of certain stars.

After about twenty years, a new theory was advanced and adopted by most of the physicians and historians of the time. It was said the disease was contracted in the New World by the followers of Christopher Columbus, that they had carried it to Spain, and there communicated it to women with whom they had sexual intercourse.

At the commencement of the sixteenth century, the navigators and merchants who followed the route opened by Christopher Columbus, reported that an affection resembling syphilis had existed from time immemorial in the newly-discovered islands, and that it was cured by drinking a decoction of guiacum, which grew in abundance in Hispaniola.

The following propositions contain the substance of Oviedo's ɔpinions with regard to the importation of syphilis, published in 1535:—

1. Syphilis is indigenous to the islands and continent of America, where it had existed from time immemorial.

2. It is cured in America with a decoction of guiacum.

3. This disease, which is contagious, was brought from America to Spain when the followers of Columbus returned after their first or second voyage.

4. The disease was afterwards carried from Spain into Italy by the soldiers which Gonzalvo de Cordova led in 1495 into the kingdom of Naples to assist Ferdinand, whose country had been invaded by Charles VIII.

Fracastor is the only contemporary of Oviedo who objected to the theory of the American origin of syphilis. He believed in astrological predictions, and also declared it impossible that a disease could appear in so many countries of Europe at the same time. M. Bassereau considers this last objection of no weight. Syphilis, he says, was carried gradually northwards between the years 1494 and 1496 by the French, English, and Scotch mercenaries, who composed a considerable portion of the armies at that time struggling in Europe.

Professor Mosler recently read before the Greifswald Medical Society an interesting paper (reported in the *Berlin. Klin. Woch.* for 10th November) on the "Utility of the Introduction of Large Quantities of Fluid into the Intestinal Canal in the Treatment of Internal Diseases." He observes that Professor Gustav Simon not long since demonstrated that water forcibly injected by the anus very speedily traversed the large intestines, probably reaching the small intestines, without doing the patient the slightest harm. In two patients having fæcal fistulæ, tepid water forced in by a pump flowed out from the apertures within five minutes. Since then, Dr Hegar has substituted an irrigator for the pump with the same result. The apparatus consists only of a glass funnel and a caoutchouc tube a foot and a half in length, furnished with an olive-shaped mouth-piece. Dr Hegar states that with this apparatus an enema may be readily administered while the patient lies on his back or side; but if it is wished to fill the upper portion of the intestine, great advantage is derived from placing him on his elbows and knees, or in a position which brings the head and chest forcibly towards the pelvis. Having cleared out the tube of air by filling it and the funnel with water, it is introduced into the anus, the funnel being held on a level with this or a little higher. The first pint of fluid usually passes readily enough, but at the third or fourth the passage becomes slower, and requires the funnel to be raised. The introduction should be made very gradually, and even temporarily suspended by lowering the funnel, which, indeed, must never be raised higher than necessary to secure the passage of the fluid. Dr

Hegar has never had occasion to raise it more than half or three-quarters of a German foot above the anus, and has been enabled to introduce from five to nine pints or more before the sphincter yielded, and allowed the fluid to flow out again by the side of the tube.

Professor Mosler has been enabled to confirm these statements by observations in his own clinic, having been able in this way to introduce four or five litres of water. He has, however, not infrequently found that much headache and inconvenience are caused by the head being kept so long in a dependent position; and he thinks mischief attends the patients assuming the knee-and-elbow position in cases of fever, affections of the head, and great debility. These and other reasons, which we pass over, induced him to try whether this inconvenient position might not be avoided, and the intra-abdominal pressure which Hegar sought by its means to overcome, be obviated by the employment of a longer tube and a higher elevation of the funnel. The first trial was quite successful, three litres of warm water having been introduced in fifteen minutes without any returning. The passage of the water through the longer tube and with the more elevated funnel took place much more rapidly, and in order to prevent the too rapid distension of the intestine, the fluid was only poured through the funnel gradually, while the stream was often interrupted by compression of the tube. The more rapidly the fluid is introduced, the more rapidly will it flow out again; but three or four litres of warm water, very carefully and slowly introduced, are retained by many patients for more than three hours; in others its discharge takes place at once, but the inconveniences produced by the distension of the intestine then soon disappear. In no case has any abiding ill effect occurred; and the most obstinate constipations, which have resisted all internal and external means, have frequently been relieved.

In all cases in which he has thus introduced from three to four litres of warm water, while the patient has been lying on his back, Professor Mosler has been able to convince himself that the ileo-cæcal region—which, prior to the injection, yielded a tympanitic resonance—then gave a decidedly dull sound on percussion, so that he and his colleagues felt able to conclude that the water had reached the cæcum. He tried many experiments with coloured water on the dead body, but these yielded no results, for, owing to the absence of tone, the intestinal canal is able to take in a less quantity of fluid after death than during life. A discussion on the subject took place at the late scientific meeting at Wiesbaden, where many denied the possibility of this penetration of fluid unaided by the pump. However, the matter was soon tested effectually in a case of fæcal fistula in Professor Simon's clinic at Heidelberg. The sac of the cæcum and the valvula Bauhini could be plainly felt by the finger. When a litre and a half of warm water was allowed to pass from an irrigator raised two feet in height (the patient being on his back), so quickly did it pass through the large intestine that in two minutes it streamed out of the fistula, the patient suffering no inconvenience. A tube, about two feet in length, and Hegar's glass funnel constitute the simplest apparatus, costing, with a cock attached to the tube to regulate the flow, only three or four shillings.

Among the indications for the employment of this means are—1. The removal of various forms of intestinal obstruction, especially when due to retained fæcal masses. Numerous examples of the striking success with which these are detached after resisting various other means are cited; and stercoral colitis and typhlitis are in the same way prevented or relieved. 2. Professor Simon is of opinion that in internal strangulated hernia—in hernia retro-peritonealis and diaphragmatica, and indeed in the early stages of all hernias—this means is often highly useful. 3. In many affections of the large intestine, and especially dysentery, it is of great value,—enabling us to thoroughly and frequently wash out the gut, and inject astringent and antiseptic remedies. 4. In intestinal hæmorrhage, the careful introduction of iced water is well worthy of attention. 5. Various forms of icterus—especially the catarrhal and cholelithiasis, affections often attended by vomiting which precludes the use of medicines—may be benefited by large injections of water, which stimulate the secretion of bile, and favour the expulsion of catarrhal accumulations and gall-stones. 6. In helminthiasis also these injections are of use, assisting, when combined with warm milk, in dislodging tænia, and in bringing away the oxyuris vermicularis which may occupy not merely the rectum, but the entire tract of large intestine.—*Medical Times and Gazette*, 3d Jan. 1874.

DOUBLE SPLEEN AND KIDNEYS. By Surgeon-Major G. W. JAMESON, Civil Surgeon, Ghazeepore.—The following is an extract from the notes of a post-mortem examination performed on the body of Bickhoo, resident of city of Ghazeepore, on the 28th of Oct. 1873.

In addition to one healthy well-developed spleen, there was a *second* smaller one, connected with the abdominal vessels by separate communications of its own, and situated between the ordinary spleen and the liver. The smaller was of a roundish shape, and had a distinct hilus.

Weight of 1st spleen	9 oz.	1 dr.	6 gr.
„ 2d „	1 oz.	1 dr.	30 gr.

Besides the above abnormality, there were four kidneys; two of these were well developed, healthy, and in the usual situation, while the second pair were small, intensely inflamed, and situated lower down than the others.

The four kidneys had each their separate arterial and venous attachments and ureters.

Weight of the two normal kidneys .	5 oz.	4 dr.	
„ „ smaller „ . .	1 oz.	4 dr.	24 gr.

The bladder was exceedingly small, walls much hypertrophied, and the mucous coat somewhat inflamed.—*Indian Med. Gaz.*, Jan. 1874.

CASE OF MADURA FOOT—MYCETOMA. By Surgeon K. M. DOWNIE, M.D., Agency Surgeon, Ulwar.—The pathology of Madura foot has lately excited some attention at home. I therefore forward this note in the hope of eliciting opinions and observations from those who have met with a considerable number of cases.

On 19th May, a man of about thirty years of age was admitted to the Ulwar Dispensary. His left foot was very considerably enlarged, and presented numerous diseases, some of which implicated the ankle-joint. From these openings proceeded a thin discharge, frequently or usually containing black gritty particles. Severe local pain was complained of, and the patient's appearance was worn and somewhat cachectic. He stated the disease to have commenced about three years previously. Amputation was performed at the lower third of the leg on 30th May. The man made a good recovery.

After removal the diseased foot presented to the naked eye appearances very similar to those in a preparation in the Netley Museum, noted as a typical specimen. The synovial membrane of the ankle and intertarsal joints was thickened and gelatinous in appearance, while the bones were softened so as to admit of section with a knife without the use of great force. There were numerous granular masses of black material in close relation to the joints and involving the bones; these were tolerably defined and associated with some caseous matter, but not apparently contained by any investing envelope.

I forwarded some of the morbid material to Dr D. D. Cunningham, who very kindly examined it, but could find no trace of fungal life; nor, in as far as his experience goes, is the presence of a fungus ever other than accidental. I have asked him to be good enough to add a note of the microscopical appearances observed by him.

Note.—The material referred to was carefully examined, both by Mr T. R. Lewis and myself. The specimems abounded in fatty crystals, oil-globules, and myelone-tubes, presenting many appearances which might very readily have been ascribed to the presence of fungal elements, until their true nature was revealed by the use of appropriate reagents; but neither spores, mycelium, nor cells of a really fungoid nature were detected in any instance.—D. D. C.—*Indian Medical Gazette*, 1st Dec. 1873.

[This case, written by one distinguished graduate of the Edinburgh school, and with the microscopic report by another equally well known and distinguished, is also a very interesting one in itself.—ED. *E. M. J.*]

COMPLETE REMOVAL OF THE LARYNX.—Professor Billroth, on 31st December removed the entire larynx with the epiglottis, an operation never before ventured upon, although Dr Czerny, by a careful series of experiments performed in 1870, had shown that it was anatomically and physiologically practicable and justifiable. Professor Billroth intends to publish an extended account of the case after its complete issue has been determined; and in the meantime the following particulars have appeared in the *Wiener Med. Wochenschrift* of 10th January:—The operation was resorted to, for the purpose of prolonging the life of a man forty years of age, who was suffering from carcinomatous growths in the larynx. Dr Störk had several times relieved him from impending suffocation by the removal of portions of these growths with the aid of the laryngoscope; but at the beginning of November the growths had extended so far within the larynx that their removal in this manner was no longer possible. Professor Billroth therefore performed Balassa's operation of "laryngo-fissure," and, having cleared out the interior of the larynx, applied the liquor ferri to this. The result of this operation, which was remarkably well borne, was at first most successful, but by the middle of December new growths appeared, and by the end of the month fearful paroxysms of suffocation again recurred, rendering the retention of a canula necessary. Here, according to ordinary rules, therapeutical interference would cease, the patient being rendered able to breathe perhaps for some months longer, until the growths had rendered the continuance of life impossible. When Professor Billroth, on 31st December, determined to make one more attempt to aid as far as possible the patient, who was yet a strong man, he intended to repeat the operation of laryngo-fissure. Finding, however, with Dr Störk, that the entire larynx was filled with cancerous masses, and that the vocal cords were totally destroyed, and believing that the projected operation could only be incompletely executed, he determined upon the more radical procedure of removing the entire larynx, encouraged by the fact that the disease was limited to the larynx, the neighbouring glands not having as yet become affected. The patient bore the operation very well, and breathed freely and directly from the trachea, in which a canula was placed. There was but little fever, and that of short duration; and at the date of this communication the wound had contracted greatly, and was rapidly healing. Drs Billroth and Czerny are now engaged in devising the construction and application of an artificial caoutchouc larynx, which will restore to the patient the power of speech.—*Medical Times and Gazette*, 14th Feb. 1874.

GUNSHOT WOUND OF THE CEREBELLUM—ABSENCE OF SPECIAL SYMPTOMS.—A man, when maniacal from pecuniary losses, shot himself with a revolver behind the ear, on a bench in a public garden. He was taken to hospital, and placed under M. Broca's care. Twenty-four hours after, his state is thus described:—He was calm, lying on his back, wandered in his speech, but preserved his memory, spoke freely, and all his senses seemed uninjured. Sensation was unaltered, and there was no paralysis of bladder or rectum. Pupils were normal, and tongue protruded straight. He had no vomiting. He could not be tested as to his walking; but he had walked a little on his way to hospital, and as far as could be observed there was no difficulty whatever in co-ordination of movements. In a word, there was not a single symptom of brain injury. There was a small wound behind the ear where the ball had entered; there was no aperture of exit. Next day there were no alarming symptoms; but towards evening meningitis set in, which proved rapidly fatal.

The absence of cerebral symptoms opened the question if the ball had ever penetrated the skull, and had not been arrested in the bone or meninges. The autopsy solved the problem. The ball had penetrated the temporal bone at the base of the masseteric eminence, had crossed the lateral sinus, and reached and fractured the opposite occipital wall, passing through the cerebellum in a curved direction, through its widest axis, without injuring the fourth ventricle. The ball was in two unequal fragments, probably broken in its contact with the masseteric eminence. Two facts are learned from this case—1. The absence of every functional symptom of an injury so extensive of the cerebellum. 2. An equally remarkable one—the irregular line of passage of the ball through a homogeneous, uniformly soft substance like the brain. This seems to have been caused by the irregular force given to the ball before it entered the brain substance.—*Gazette des Hôpitaux*, No. 147, 1873.

DANGER OF SHARP-POINTED FOREIGN BODIES IN THE BLADDER.—A man, aged 34, introduced a pencil into the urethra; it escaped, and entered the bladder. Two days afterwards, he was brought into hospital in a state of great danger. The abdomen was distended, and scrotum, penis, and perineum were much swollen. M. Peau operated by median lithotomy, and extracted the pencil entire. The patient died in two or three days of an acute peritonitis; and the examination showed a small perforation of the fundus of the bladder, extending completely through its wall, and giving rise to infiltration of urine and peritonitis.—*Union Médicale*, August 1873.

ABORTIVE TREATMENT OF BOILS.—The following, applied to boils with a camel-hair pencil or feather, gives great relief in a very short time. The inflamed surface, and a little beyond all around, should be painted with the medicine every fifteen minutes, or as fast as it dries, till a good thick coating covers the part. The throbbing, tensive pain, and the intense tenderness, will be promptly relieved; the redness will subside; the smooth, shining integument will shrink and become wrinkled, and comfort will succeed torment. If the boil is in the first stage it will disappear without slough. If slough has already formed it will be quickly separated, and the cure soon complete:—℞ Tinct. arnicæ, ʒj; acidi tannici, ʒss; acaciæ pulv. ʒss. M. It should be used as soon as prepared.—*C. B. in Cincinnati Lancet and Observer.*

Part Fifth.

MEDICAL NEWS.

Royal College of Physicians.—At two receptions, held by the President and Council on the evenings of the 5th and 12th March, Dr M'Kendrick delivered two lectures on "The Present State of our Knowledge regarding Sensory Impressions." Both lectures were fully illustrated by diagrams and by histological preparations of the various terminal organs, examined under high magnifying powers. In the first lecture, the structure of the terminal organs of the senses of touch, taste, smell, and hearing was described. The recent observations of such histologists as Max Schultze, Waldeyer, Rudinger, Thin, and others were referred to, and were considered especially in their bearing upon our knowledge of the functions of the ultimate recipient apparatus for each sense, and our conception of how these functions are performed. In the second lecture, after alluding to the speculations of John and Charles Bell regarding the existence in the cochlea of fibres of various lengths, capable of vibrating synchronously with musical tones of various pitch, the lecturer described the theory of Helmholtz as to the functions of the pillars of Corti. Recent histological inquiries having shown (Waldeyer) that the pillars of Corti constitute a supporting and not a vibrating apparatus, the lecturer alluded to the hypothesis of Hensen, as to the function of different segments of the membrana basilaris. Dr M'Kendrick then illustrated experimentally, by means of a special apparatus, most of the leading principles regarding the intensity, pitch, and quality of musical notes developed by Helmholtz, in his "Tonempfindungen." Lastly—after briefly alluding to the delicate observations of Max Schultze on the ultimate structure of the rods and cones of the retina—the specific action of light on the electro-motive condition of the retina and optic nerve (which has formed the subject of a joint investigation of Mr James Dewar and the lecturer) was experimentally illustrated by means of a reflecting galvanometer and lime-light. The lectures, which were exceedingly interesting and illustrated by many experiments and preparations, were listened to by a large audience with great attention. We are glad to say that they will be published in a condensed form in this Journal, in future numbers.

Action for the Fees of a Consultant in a Midwifery Case in France.—The following is a brief note of a trial of some interest which had been decided by the judge with great sense:—Madame Bourdon had engaged Dr Coqueret to attend her, for a fixed sum of 200 francs. The case had been tedious. Dr Carpentier was called in and applied forceps, at Dr Coqueret's request. Bourdon paid the 200 francs, but refused to pay 80 francs demanded by Dr Carpentier for the operation, and 10 francs for two subsequent visits. This refusal was on the plea that Dr Coqueret, and not he, had requested the consultation. The judge, after consultation with medical men and others, decided in favour of Dr Carpentier, holding that the fixed price had only been on the understanding that the labour was a simple and natural one, and that the assistance of another medical man, and the performance of an operation, demanded an extra fee, and that in common fairness this should be paid by the person receiving the benefit.—*La France Médicale*, No. 10, 1874.

THE following items are extracted from an elaborate New Tariff of Fees at Lyons:—

Visits.	Rich.	Easy.	Labouring.
A single visit or consultation, .	8s.—16s.	4s.— 8s.	2s. 6d.—4s.
Night visit, 10 P.M. to 7 A.M., .	16s.—32s.	8s.—16s.	5s. —8s.
Distance per mile, additional, .	5s.	4s.	2s. 6d.
Ordinary labours, abortions, etc.,	£4—£8.	£2—£4.	25s.—£2.
Consultations with another, *for each*,	25s.—£2.	25s.	£1.
Remaining at bedside, except at labour, per hour, . . .	16s.—25s.	8s.—16s.	2s. 6d.—5s.
Certificates, ordinary, . .	8s.—16s.	4s.— 8s.	2s. 6d.—4s.
„ medico-legal, . .	10s.—20s.	8s.—10s.	4s.
Fractures, ...	£4—£8.	£2—£4.	£1, 5s.

—*Lyon Médicale*, No. 1, 1874.

CORRESPONDENCE.

GASTROTOMY IN EXTRA-UTERINE PREGNANCY.

To the Editor of the Edinburgh Medical Journal.

SIR,—It seems to me a most extraordinary thing that M. Depaul, Professor in l'Ecole de Médecine, should be so profoundly ignorant of the medical literature of his own country, that he should have attempted to remove the placenta in this operation, and so have directly contributed to the loss of his patient. Still more wonderful is it that Dr De Ranse should publish such a sentence as the last one of the extract from the *Gazette Médicale*, which appears in your Journal of this month—"In a word, an almost certain death as the result of a placenta left in the abdomen ought to warrant any possible effort for its removal"—when M. Koeberle of Strasburg, and Dr Keller of Paris, and in this country my own success, and that of my colleague, Mr Ross Jordan, have shown conclusively that the placenta ought never to be disturbed, but left to separate; for this separation has been accomplished safely in all four cases where it was left to natural processes, and it has been fatal in *every* case where it has been done by the hands of the surgeons.

It would almost seem as if the Parisian surgeons were as ignorant of French, as they certainly and admittedly are of English, surgery. —I am, etc., LAWSON TAIT.

BIRMINGHAM, 20*th February* 1874.

OBITUARY.

THE LATE DR KILGOUR.—There has recently passed away from among us a member of our body, whose name and memory will long be cherished in the North. We mean the late Dr Alexander Kilgour of Aberdeen. He was to Aberdeen what Dr Abercrombie (also an Aberdeen man) was to Edinburgh. And indeed, as professional men, the two had many features in common,—both of them being distinguished by great natural shrewdness and tact.

Dr Kilgour was essentially the architect of his own fortune. He owed little or nothing to adventitious favouring circumstances. His sole patrimony lay in an excellent classical and general education, which he received first at the Grammar School of his native city, and afterwards at the Marischal College and University, at which, on the completion of his curriculum, he graduated M.A. But to a man of his gifts and energy, this patrimony was fortune enough. It enabled him to rise to the top of the ladder, and acquire fame and wealth,—and an honoured name. He was a man of great natural ability. Shrewd he was beyond most men; and he possessed in uncommon measure that highest, if not also rarest of all gifts, common sense. Artifice he had none. He was natural in all his ways, and he scorned the tricks and bypaths by which some men strive to push themselves on in life. He owed the place he attained as head of the profession in the North to his own merits and his own force of character; and none grudged him his position.

His medical education he pursued chiefly in Aberdeen, but in part in London. He passed the Royal College of Surgeons of England in 1826,—graduating subsequently (1833) in Medicine at Marischal College. In 1826 he began practice in Aberdeen, keeping for a few years an open surgery,—or, to speak plainly, a *'Pothecar's shop*. Soon after he was appointed Demonstrator to the Lecturer on Anatomy in the then joint Medical School of the two Universities. This post he did not hold long; but his tenure of it enabled him still further to perfect his knowledge of that foundation-branch. In 1828 or 1829 he became an independent or extramural lecturer,—teaching Materia Medica and Therapeutics. The present writer attended his course in 1829, and he well remembers the admirable manner in which it was conducted.

In 1831 he was elected one of the Medical Officers of the General Dispensary. Here he devoted himself with characteristic energy to the duties of his office; and here he may be said to have laid the foundation of his future professional success. In 1838 he was appointed one of the Physicians to the Royal Infirmary, and, specially, Lecturer on Clinical Medicine,—succeeding the late Dr Alexander Murray (the eminent botanist), who was the first holder of that lectureship, as distinguished from Clinical Surgery. For, previously, in Aberdeen, clinical medicine and clinical surgery had been taught together, and by one lecturer.

Dr Kilgour now stood fairly on his own feet. He had gained the position which was all that a man like him needed, to secure his reaching in due time the top of the tree. In a very short time he made an enviable reputation for himself at the Infirmary, both as a physician and a teacher; and there he began and reared an excellent pathological museum. He threw himself with might and main into his hospital and clinical work, and in a few years he stood confessedly the foremost medical man in Aberdeen and the North. His practice, as well general as consulting, was steadily increasing; and when in 1850 the late Dr Benjamin Williamson died, Dr Kilgour at once came by acclamation to the front. Ever after he retained that position; and indeed the range of his practice came far to exceed that of any previous physician in Aberdeen. No doubt, the rise and rapid extension of our railway system widened largely the sphere of his practice. But it was large beyond precedent, independently of this.

We have said that, shortly after settling in practice, he started as a lecturer on his own account. In 1839–40, on the breaking up of the joint Medical University School, and the setting up of two separate schools,—the one in connexion with the King's College and University, the other with Marischal College,—Dr Kilgour became Lecturer on Pathology and Practice of Medicine in the former. This office he retained till 1848, when the demands of his rapidly-increasing practice compelled him to relinquish it. On retiring from it, the University conferred on him the honorary degree of M.D.

Dr Kilgour shone as a teacher, both at the Medical School, and also at the Infirmary; and he was deservedly popular with his pupils, albeit he never courted popularity. There was a clearness and a directness in all he said, and a wise regard to the relative values of different subjects, and the practical bearings of things. And his lectures were often enlivened with sallies of genuine wit and humour. He made no display of a knowledge he did not possess. He did not affect to be learned or profound. He did nothing for "class-effect," but everything in a natural, homely way to impart wholesome instruction, scientific and practical. His heart, however, lay much more in the wards of the Infirmary, in the clinical theatre, and in the post-mortem room, than in the college class-room. For, in truth, he set small store by systematic lectures on physic; and he often spoke with derision of the daily lecture system, peculiar to Scotland, and of the folly—for such he deemed it—of spending and mis-spending so much valuable time, as it entails, on the delivery of formal lectures (not always listened to) on not a few branches which may be as well or better learnt at home from books, and from which teachers themselves draw most of their materials.

The general tone or habit of his mind as a practical physician, cannot be better depicted than is done in a notice of him in the *Scotsman.* "While he knew and practised every proved new artificial method of investigation, such as the stethoscope, microscope, etc., he relied mainly on the means that are common to all men—the natural unassisted eye, and ear, and touch, the steady observation of superficial symptoms—which may be called the vernacular language of disease. Like all great doctors, he knew not less what drugs could not do than what they could; and he was justly celebrated for the intuitive sense he had by long practice acquired, of what his art could not do, trusting to the recuperative powers of nature and management, and the removal of what the old masters called the *lædentia*, more than to any *quasi* specific or heroic treatment." With Alison, he held that drugs and other appliances are "only auxiliary to the provisions of nature for the spontaneous cure of disease;" and with Gubler, that "l'organisme se guérit lui-même," the office of the physician being, simply, "le placer dans de conditions favorables au retour d'un mode de fonctionnement régulier"—or, as some one puts it, "Natura sanat, medicus curat, morbos." These were Dr Kilgour's therapeutic beliefs, and the basis of his practice,—the essence of which was simplicity and directness,—and for the rest, hope and expectation, or smoothing the passage to the grave.

We have as yet spoken of Dr Kilgour in his professional capacity only. Yet he was much more than a physician pure and simple. He was a man of refined tastes, and well versed in the fine arts. He was an accomplished classical scholar; and he retained through life his love, early acquired, for the poets, philosophers, and historians of ancient Greece and Rome. He was familiar with the writings of Hippocrates and other ancient medical celebrities in their native tongues. He seldom went on his journeys to the country without "his Horace, Seneca, or Erasmus," or his Sydenham. *Nulla dies sine linea* was the habit of his life in respect of those worthies. Moreover, while he ever kept himself well abreast of the scientific and practical literature of his profession, he diligently cultivated general literature, and was in every sense a well-informed man. Wordsworth was his favourite among modern poets. He did not himself contribute much to the literature of his own profession. Saving only an occasional paper to this Journal, he only published one work of any pretensions. This was his "Lectures on the Ordinary Agents of Life, as applicable to Therapeutics and Hygiene." It appeared in 1834, when he was still young. It was well received and favourably reviewed. Its style is terse, vigorous, and racy; and it contains much valuable information—valuable even at the present day.

But besides being a professional man, and a man of high and varied mental culture, Dr Kilgour was also a man of "affairs." He was a keen politician. He took a lively interest in public matters, national and civic. And ranging himself on the Liberal side, he once and again, indeed often, in his earlier years especially, made his voice to be heard and listened to on the platform and in the open air. He took and held a foremost place among the reformers of 1832. He was a Liberal when Liberal principles were as yet scarcely in the ascendant. He was, in fact, a Whig and something more. This, as may readily be imagined, in one so young, and with no adventitious "backing" in respect of birth or fortune, was not conducive to his professional advancement. It "sore let and hindered" it. His impassioned advocacy of Liberal men and measures, and his equally impassioned denunciation of abuses, imperial and civic, kept him out of place and kept down his practice. His rise after a time was singularly rapid; but this he owed to his own intrinsic merits and his own force of character. The veriest Tory, when in an agony of gall-stone or gravel, will not heed the politics of his doctor if only he have faith in his skill. And so it came about, that Dr Kilgour made way in all quarters, in quarters even where his political creed was detested. Yet it needed time. And as showing how small his practice was for the first ten or twelve years of his life, and how much public affairs engrossed his regards, the present writer may remark, that he has repeatedly heard Dr Kilgour say, that if he had but £200 a year he could call his own, and could rely on for the rest of his days, he would cut the the profession to-morrow! Often in after years did he smile when reminded of this declaration of his younger years. For, during many years subsequently to this, his professional earnings could not have yielded him less than £2000 or £3000 a year; and he is understood to have amassed an ample fortune.

But we must pull up. The space at our command is well-nigh exhausted. A few words more, however, will suffice.

Dr Kilgour, it must not be omitted to note, took a warm interest in all the public institutions of the town. He took an active part in the establishment of the Hospital for Incurables, to which he gave his professional services gratuitously. But it was its educational concerns—its Grammar School and its University—that mostly called forth his energies. On occasion of the appointment to the office of Lord Rector in Marischal College being found to lie in the hands of the students, he exerted himself actively and successfully, while yet a student in Arts, to secure the election of the late Mr Joseph Hume; and subsequently, when a medical student, of Sir James M'Grigor; and ever after, he strove with his voice and by his pen to promote the advancement of University reform. He was a decided advocate for the union of the two Universities in Aberdeen—a union effected in 1859. But he strongly reprobated, and always deeply regretted, the merging of the two Faculties of Arts in one, and the transference of this to King's College. On the creation of the University Court under the new arrangements, he was elected the representative in that body of the General Council.

Three years after, his party having been caught napping, he lost his election. On the next election, however, he was again returned to the University Court, in which he rendered important services to the University. And he would have continued a member of the Court had he not been compelled to retire from it.

Alas! for the cause. It was failing health. In the summer of 1870, when yet only sixty-six years of age, and when we hoped to have him with us for many years to come, serving his day and generation, as he had done heretofore so well and so nobly, Dr Kilgour was seized with a serious and a painful illness, from which he never recovered. Up to that period, he had enjoyed uninterrupted good health. He was an early riser, and for many years a steady bather in the open sea almost all the year round. He was, in fact, a singularly robust, active, and vigorous man, and gave promise of attaining to a good old age. But, alas! it was otherwise ordered. Everything that professional skill and care could do for him was done, but to little purpose. Feeling that his race was run, that the battle of life was over, he relinquished all public appointments, and retired to his country-seat of Loirston, in Kincardineshire. There he spent the remaining days of his life, coming to town occasionally, and readily giving advice, when able, to old patients and friends. During the last year, and especially the last few months of his life, he was reduced to a state of great weakness, seldom leaving his bed, and suffering at times severe bodily anguish. He had long been desirous to have his release; and this was granted him on the 19th of February last. He was born on 28th October 1803. So that at his death he had completed the term of seventy years.

This short and imperfect sketch of Dr Kilgour must suffice. There is much of great and enduring interest attaching to his "life and conversation," which we have not space to enter on. He was altogether a self-made man, and a man of mark. Had he entered Parliament, as was once and again in the view of his friends, he would doubtless have made a name for himself there. Singularly gifted he was by nature, and he laid out his gifts to advantage. He did not hide his talent in a napkin. He was no trifler, taking things easily. He ever strove to do his duty in that state of life unto which it had pleased God to call him. He enjoyed life, too, heartily, and intercourse with society. He was always a welcome guest at the dinner-table, and the soul of it. For nearly forty years, he made it a matter of conscience to attend the anniversary dinner of his brethren of the Medico-Chirurgical Society—the whole history of which and of its founders he knew better than any one else, and of which he was twice elected president. Somewhat blunt he was in manner; but this was natural to him, and naturalness was the habit of his life. True, downright honesty was ingrained in him. He said what he thought, and he did what he deemed right, regardless of fear or favour. He hated shams, pretence, and divisive courses. He won his renown, and he rose to the position he attained by reason of qualities, native and acquired, that drew towards him, unconsciously and irresistibly, the regards, and the confidence, and the admiration of all he came in contact with—his patients, his pupils, his professional brethren, and the public. Alas! that he should not longer have been spared to us.

> "Quis desiderio sit pudor aut modus
> Tam cari capitis?"

Dr Kilgour was twice married. His first wife was Miss Marjory Dyce, daughter of Alexander Dyce, Esq., of Tillygreig, in Aberdeenshire. She died in childbed of her firstborn, which was also stillborn. He afterwards married Miss Mary Elizabeth Duncan, daughter of Thomas Duncan, Esq., Advocate in Aberdeen, and whose widow married the late Harry Leith Lumsden, Esq., of Auchindoir and Clova, in the county of Aberdeen,—a man of note among the Whig county gentlemen. By his second wife, who survives him, Dr Kilgour leaves one son, Alexander, the heir to his estates of South Loirston and Cove, in Kincardineshire.

DEATH OF CRUVEILHIER.—Another of the great names which adorn the medical literature of France has passed away at the good old age of 83. Cruveilhier's was a European reputation, and his name has ever been identified with the subject on which he wrote his thesis for the Doctorate of Paris in 1816, viz., Pathological Anatomy. His great work on that subject has been, for nearly two generations, a sort of quarry or reservoir, from which other labourers in the same field have hewn or drawn material on which to found their own superstructures; and the great atlas of pathological drawings which illustrates the work has not yet been equalled in size or completeness. His hospital work was incessant, painstaking, and successful. He was no mere pathologist; he was also an able clinical physician,—witness his labours on purulent infection, on progressive muscular atrophy, and many other important subjects. He had a large and lucrative practice; and we learn from the notices of his life in the Parisian medical papers, that his liberality and kindness to the poor were very great. For some years, age and labour had begun to tell upon him, and he died on the 6th of March, at his country seat near Limoges, having enjoyed, like his northern fellow-labourer, Kilgour, the repose and calm retreat he had worked for so well during the latter years of a well-filled life.

DR NEIL ARNOTT, F.R.S.—By the death of this well-known physician, another of the links of the present to the past century is snapped. Born in 1788, after a short period of travel in the East India Company's Medical Service, he settled in practice in London in 1811; and, during his long residence in that city, has made himself a name not only as physician, but also as a scientific writer of great zeal and energy, and an inventor of many useful and practical improvements in surgical appliance. His work on the Elements of Physics is of great value, and it has passed through many editions and translations. The Arnott stove, the water bed, will always be associated with his name; and not the less to his honour, though not to the advantage of his pocket, he always refused to patent his inventions. He was equally liberal in his donations of bursaries to the Universities of his native country, in aid of the study of experimental physics.

Forbes Winslow, M.D., D.C.L.—Forbes Benignus Winslow was born in London in August 1810. His early education having been obtained in Scotland, he was enrolled as student of medicine in the Middlesex Hospital at the early age of fourteen. Even in his student days, the subject of insanity seems to have had a peculiar interest for him, as we find him at twenty-one contributing a paper entitled an "Essay on the Application of the Principles of Phrenology to the Elucidation and the Cure of Insanity." He became a member of the Royal College of Surgeons in 1835, and in 1849 obtained his degree as Doctor of Medicine in the Aberdeen University, and began general practice in Hertford Street, Mayfair. A direct descendant of Edward Winslow, one of the Pilgrim Fathers who sailed in the Mayflower, he seems to have inherited the strength of will and perseverance of spirit of his Puritan ancestors. Finding his means in the early years of his practice inadequate for the support of his family, he became parliamentary reporter for the *Times* newspaper, and must indeed have possessed an iron constitution to have been able to combine, as he did, the nights of toil spent in the reporters' gallery, with the long days of harassing professional labours. From the time Dr Winslow settled in Cavendish Square, and became proprietor of two private asylums at Hammersmith, his career was one of unvarying success. Acknowledged as an expert witness in courts of law where the question of insanity was mooted, he gave evidence on the trials of Mrs Vyse and Mrs Brough, and many others, the trial of Townley being the last in which he appeared. The bent of all Dr Winslow's writings was to improve the condition of the insane, and we are glad to think his humane efforts in that direction have met with the success they merited. Of Conservative principles, Dr Winslow was offered respectively the representation of Cambridge, Norwich, Maidstone, and Barnstaple, but on each occasion refused the honour, on the plea of his arduous professional labours. He died at Brighton on Tuesday, 3d March, in his sixty-third year. Of a truly philanthropic spirit, none ever sought his aid in vain, or turned away disappointed from an appeal to his unfailing generosity.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Excision of the Anterior Tarsus and Base of the Metatarsus—a New Operation.* By PATRICK HERON WATSON, M.D., F.R.S., F.R.C.S., Senior Surgeon, Royal Infirmary, etc.

(*Read before the Medico-Chirurgical Society of Edinburgh, 1st April.*)

FOUR years ago, John Scott, a lad æt. 19, was recommended to my care by Dr John M'Dougall of Galashiels, suffering from disease of the anterior portion of the tarsus. The disease was spontaneous in its origin, subacute in its progress, involving the articulations chiefly upon the inner side of the foot, between the cuneiform bones and metatarsal bones, but accompanied with symptoms indicative of involvement of the external tarso-metatarsal joints and of the scaphoid and cuneiform bones in a minor degree of the same process. The pain was such, on movement or pressure, that the lad was unable to walk or work. Under rest in bed, blistering, and constitutional treatment, the symptoms gradually quieted down; and though the malady was obviously only in abeyance for the time being, the condition of the parts permitted of the employment of the application of the plaster-of-Paris apparatus, so as to afford support and secure rest. Wearing this apparatus, the patient went to the country, but again returned to town after the lapse of a few months with the symptoms where they were, and urged that some means should be adopted by which his limb should be restored to such measure of soundness as would admit of his following his employment.

To amputate the foot seemed too severe a measure to be justified by the condition of the parts or the circumstances of the patient; and although no collective abscess had formed in the soft parts, there could be no reasonable doubt that suppuration had already commenced within the bones and joints involved. It was in those circumstances that it appeared to me that I was justified in undertaking an operation, which I then devised, and which I have now the honour of submitting to the Society.

It was obvious that all the disease which required the intervention of art was confined between the base of the metatarsus in front and the astragalus and os calcis behind, and that the excision of such a section of the hard structures forming the skeleton of the foot as was bounded by the two transverse lines of Chopart's line of amputation behind, and Hey's in front—that is to say, the scaphoid, cuboid, and cuneiform bones, and bases of the metatarsal bones—would secure the fulfilment of every requisite for sound recovery, in so far as the removal of all actually existing osseous disease, and all tension of the articulations, were concerned. There remained, however, still the question, Whether the parts left behind would consolidate soundly? And even could this be answered in the affirmative, a doubt remained whether the foot thus reconstructed would be either seemly or useful. One feared that the remaining metatarsal portion of the foot and the toes might become distorted, so as to be more a hindrance than a help in walking, and that the os calcis and astragalus, should they undergo a rotation within the soft parts, as is observed in Chopart's amputation, might occasion ulceration of the soft parts corresponding to the point of tread, in consequence of disease becoming established in the anterior articular surfaces of these bones. In contemplating this operation, no satisfaction could certainly be obtained from the appearance presented by the parts in the dead subject after a tentative operation of this kind; the aspect of the foot being that of a Chopart's amputation, to which a flaccid pouch of soft parts was attached, by which the toes hung reduplicated upon the heel. But the experience afforded in many cases of partial excisions of the bones of the foot—as, *e.g.*, where the astragalus and os calcis had been removed, where the scaphoid and astragalus had been cut away with the extremity of the tibia and fibula, where the cuboid and two external metatarsal bones had been excised—led me to distrust the easily entertained conception that all such partial excisions in the foot must necessarily entail a permanent and unatoned-for deficiency of rigid structure, which would take the place of the osseous parts after consolidation had become complete; while the notorious impropriety of gauging the results in excision of the elbow, wrist, ankle, or knee by the appearances of these parts after the operation practised upon the dead subject, inclined me to hope better things in the case of excision of the tarsus in the living patient than the appearances in the cadaver seemed to suggest.

Furthermore, the likelihood of this fibro-osseous consolidation, together with the presence of the untouched plantar fascia, and powerful tendinous structures in the sole of the foot, led me to expect that the tendency to rotation of the astragalus and os calcis within the soft parts would be less than is observed in cases of Chopart's amputation, and that thus the risk of reproduction of the disease in the portion of the tarsal bones left behind might be commensurably diminished.

The risk of a return of disease in the os calcis and astragalus, simply because they constituted a portion of the tarsus in which e disease had commenced, did not seem to afford serious ground r concern; especially when the results of excision of single nes of the tarsus in the hands of so many surgeons had already oved the untrustworthy character of that speculative objection to l partial operations upon the tarsus, which had at one time been lvanced by a distinguished surgeon in this school, when he took xception to Pirogoff's amputation, where the mere nodule of the alcaneal tuberosity is retained in the heel-flap of the ankle-joint mputation.

These considerations seemed to satisfy me that the operation I vill now describe was justified as a proceeding amply sufficient to ffect the extirpation of all actually existing diseases of bones and oints in the foot, and of securing as a result a foot which would e more seemly, if not more useful, than an amputation at the ankle-joint. After the application of the tourniquet to the lower part of the thigh, the operation was effected by making an incision on the outer and inner sides of the foot, between three and four inches in length; that upon the outer side extending from the centre of the outer margin of the plantar surface of the os calcis as far as the middle of the metatarsal bone of the little toe, that upon the inner side of the foot from the neck of the astragalus to the middle of the metatarsal bone of the great toe. The soft parts were then carefully dissected off the dorsal and plantar surfaces of the tarsus from the outer and inner sides, until the whole extent of osseous tissues to be removed was deprived of its soft coverings. In effecting this, the thumb of the left hand formed the guide to the point and edge of the knife in keeping close to the surface of the bones, so as to avoid any interference with the important structures contained in the soft parts. A curved probe-pointed bistoury inserted between the soft parts and bones was then carried across the line of articulation, between the astragalus and scaphoid and os calcis and cuboid bones, first upon the dorsal, and then upon the plantar surface, so as to open up these joints. A little further use of the knife completed the severance of the bones posteriorly. A keyhole-saw was now introduced between the plantar soft parts and the shafts of the metatarsal bones, which were then cut through, one handle of a pair of bone forceps being inserted between the metatarsal bones and dorsal soft parts to protect the latter from injury by the teeth of the saw cutting from below upwards.

After the operation, and before removing the tourniquet, the entire wound was plugged firmly and securely by means of pledgets of lint passed through and through the gap in the foot, and laid in closely one upon the back of the other, so as completely to fill up the aperture, and, at the same time, so closely crowded together as to secure, when pressure was applied from the outside by other pads supported by a bandage, that no bleeding should take place. This dressing was retained *in situ* for forty-eight hours. When removed there was no bleeding, and the dressing was reapplied. The parts were dressed from day to day, pads of lint being placed in the cavity, with the view both of affording internal support to the soft parts, and also of securing that consolidation should take place from all the surfaces of the wound equally, without the possibility of an early closure of the lines of incision giving rise to bagging of matter and after disturbance. Externally, support was effected by two gutta-percha splints applied upon each side of the foot, external to the dressings, so as to admit of the patient getting out of bed and moving about with crutches at as early a period as possible.

The dressing was inserted into the wound for a period of six weeks, after which it was only applied superficially; the openings had not, however, closed finally for three months. During the latter portion of this period, a gutta-percha pad was bandaged against the hollow of the foot, so as artificially to maintain an arch between the posterior part of the foot and the anterior extremity of the metatarsus.

When I last saw this patient, six months after the operation, he walked smartly and well, with none of the stumping gait of an amputation. I have to-day been assured by Dr M'Dougall, that the last time he saw him, now some months ago, he walked almost without defect; and he adds, "The operation in his case I considered a great success."

Since the successful result attained in this case, I have operated in an exactly similar manner in five other cases. In all of them, the disease was in a more advanced stage, abscesses having formed, and sinuses existing. One of these cases I now present before the Society, that they may judge of the results for themselves. In this case the operation was performed upon the 23d of September of last year. The wound was, practically speaking, healed in November, and for the last two months the patient has been walking about.

Upon the last case, I only operated upon the 13th of the present month. The patient is out of bed, and doing well.

In only one of these six cases upon which I have performed this operation, have I had occasion to resort to further interference. In this case, the patient had long been the subject of disease of the tarsus, and, through vain endeavours to avoid operative interference, had become nearly exhausted. The operation afforded complete relief; but as the system of dressing the whole cavity of the wound was abandoned from the first, out of regard to the patient's sensitiveness and weakness, the lines of incision closed, and matter began to bag in the deeper parts of the wound, occasioning pain, and exciting anxieties in the patient's friends lest disease was about to return. I was induced, under these circumstances, to resort to amputation at the ankle-joint, and was thereafter chagrined to find that there was no condition which should not have admitted of sound cicatrization, had the method of dressing the cavity of the wound been rigidly carried out.

Article II.—*The Early Cases of a Typhus Outbreak.* By T. J. Maclagan, M.D., Dundee.

In Dundee typhus fever is endemic, and every now and then epidemic.

The disease being one which is universally recognised as contagious, and one which mainly affects the poorer inhabitants of the most densely-populated parts of the town, the great majority of the cases which occur are treated in the Infirmary. The records of this institution thus form the best available indication of the extent to which the disease has prevailed at a given time.

An examination of these shows that though the disease has not existed in an epidemic form since 1866, it has nevertheless persisted endemically up to June 1872.

The monthly admissions for 1872 were as follows:—

In January there were admitted 14 cases of typhus.
„ February ... 12 ... do.
„ March ... 5 ... do.
„ April ... 4 ... do.
„ May ... 9 ... do.
„ June ... 2 ... do.

From the 17th June 1872 to the 20th December 1872 not a single case of typhus fever was admitted. Thus, for a period of six months, the disease has no place in the Hospital records.

On the 20th December 1872, three cases, all members of one family, were sent in from Dudhope Crescent, a densely populated block of buildings in the centre of the town. On 23d December four more cases, all belonging to the same family as the three already admitted, were received.

On the following day, 24th December, four cases, likewise all members of one family, came from Mid Road, at the north-eastern extremity of the town, and about a mile from Dudhope Crescent.

On 26th December another case, not belonging to the same family as those formerly admitted, came from Dudhope Crescent.

On 28th December a case was admitted from Lawrence Street, at the north-western extremity of the town, one mile from Dudhope Crescent, and two from Mid Road.

In January three fresh cases came from Dudhope Crescent. In February some fresh cases were received from Mid Road. A new locality was now involved, and between the 18th February and 26th March, nine cases were sent in from the Murraygate. In April the Overgate and Cowgate became affected. In May the disease was prevalent in the streets continuous with and adjoining those already named; and in June and following months, cases were admitted from nearly all the poorer and more densely populated parts of the town. The disease was scattered all over the town, but could not be said to have been at any time widely epidemic.

The following table will convey some idea of its extent and progress:—

Month.	Number of Cases admitted to Infirmary.	Localities chiefly affected.
Dec. 1872	13	Dudhope Crescent and Mid Road.
Jan. 1873	4	Ditto.
Feb. "	9	Mid Road and Murraygate.
March "	5	Murraygate.
April "	4	Overgate.
May "	13	Overgate and Cowgate.
June "	17	Overgate, Cowgate, and scattered.
July "	18	Do., do., do.
August "	18	Do. and scattered.
Sept. "	14	General distribution.
Oct. "	14	Fish Street and scattered.
Nov. "	20	Industrial Schools (13) and scattered.
Dec. "	23	Scattered.

This table represents the number of cases of typhus admitted to the Infirmary from December 1872 to the end of 1873.

Though the admissions to the Infirmary are a good indication of the extent to which the disease prevails, they by no means indicate the full extent of its prevalence. Many cases (more than I had any idea of before I undertook this inquiry) are treated in their own homes; and often under circumstances both disadvantageous to themselves and disastrous to those around them. Evidence of this will be given in the course of this paper.

The full details of the inquiry it is unnecessary to give. It will serve every useful purpose to give only the results of the investigations made regarding the occurrence of the earlier cases, and to indicate the manner in which the disease was conveyed from one locality to another. I thus limit myself, partly for brevity's sake, and partly because in the fuller details of the inquiry there would be found no fact or inference which may not equally well be illustrated by the details given.

The localities which were first affected were Dudhope Crescent, Mid Road, and Lawrence Street; then the Murraygate, Overgate, Cowgate, and adjoining streets.

The almost simultaneous appearance of the disease, after an absence of six months, in three different and widely separated parts of the town, could not fail to attract attention. The question naturally arose, "What was the cause of the outbreak?" Did the disease arise *de novo?* Or was the poison derived from pre-existing cases? So sudden, and apparently spontaneous, an outbreak of the disease at the end of December seemed to indicate the existence of some cause, predisposing or exciting, which had not been in operation during the previous six months. The predisposing causes of typhus are, overcrowding, filth, poverty, and the miseries and depression which the existence of these entails. The exciting cause is the contagion, whose propagation in the system gives rise to the disease. Whether the former may ever give rise to the latter—that is to say, whether under certain conditions of overcrowding, filth, and poverty, the typhus contagion may be produced *de novo*—is extremely doubtful. For argument's sake, however, and for the better elucidation of our subject, we will grant that it may.

In this spirit we shall proceed to investigate the circumstances under which the disease appeared in each locality, and endeavour to find out whether, on the one hand, there existed in December 1872 any cause capable of giving rise to typhus which was not in operation during the previous six months; or whether, on the other hand, there was any source from which the cases which then occurred could have received infection.

The general result of the inquiry was to satisfy me that, so far as this outbreak is concerned, the cases were very few in which the occurrence of the disease could not be traced to a more or less direct communication, either with a previously affected individual, or with an infected locality. In only a few instances could evidence sufficient to warrant this conclusion not be obtained; but these cases bear so small a numerical relation to those in which the evidence of infection was unquestionable, and the difficulty (I might indeed say the impossibility) of tracing, in a large and dense community, the source of infection in every case is so apparent, and so well known to those who have conducted such inquiries, that the negative evidence of the few cases in which the source of infection could not be traced, does not affect or detract from the value of the positive evidence of the large number in which the disease could clearly be traced to infection, derived from a previously affected individual or locality.

Now for the details. We shall consider each locality separately. It will be unnecessary to give more than the early cases of each.

I. *Dudhope Crescent.*—This is a densely populated block of buildings, situated in the centre of the town. The earliest cases sent to the Infirmary came from here. They were a family of the name of Smith, three of whom were admitted on the 20th, and four on the 23d December.

(*a.*) The family consisted of father, mother, four sons, and three daughters—the eldest 21, the youngest 5 years old. They all resided at home, except a boy, aged 12, who resided with his grandmother in Forfar, 14 miles distant. Mrs Smith died at home on the 4th December, after an illness of twelve or fourteen days. She was said to have died of delirium tremens; but the medical man who attended her afterwards informed me that he believed she died of typhus. Subsequent events leave no room for doubt on this point. On the 4th December, then, Mrs Smith died of typhus. The disease not having been recognised as such, no steps were taken to purify or disinfect the house or bedding. Some of the family even slept in the bed in which she had died. Any doubt that remained as to the cause of her death would be dispelled by the fact that, between the 12th and the 18th of December—that is, not less than eight, and not more than fourteen days after Mrs Smith's death—every one resident in the house (Mr Smith and six children) was seized with premonitory symptoms of what turned out to be typhus. They all took ill so nearly at the same time that the infection was doubtless derived from the same source in each, about the beginning of December, the date of Mrs Smith's death. The boy who resided with his grandmother returned to his father's house on the 8th December. About the end of the month he also was attacked, had well-marked typhus, and recovered. Thus not a single member of the family escaped.

Mrs Aitken, a neighbour of the Smiths, had been in the habit of going to see Mrs Smith during her illness, and had helped to dress her body after death. She shivered on the 18th December, and was admitted to the Infirmary on the 26th; she had a smart attack of typhus, and recovered.

During the last week of her illness, Mrs Smith was nursed by a Mrs Coutie, residing at 65 Murraygate, a quarter of a mile from the Smiths' house; Mrs Coutie took ill, and died at home of typhus at the end of December or early in January.

(*b*.) In the same block of building, but in a different part of it, resided a family of the name of Stewart. The family consisted of father, mother, two daughters, one son, one widowed son-in-law, and one grandchild—seven persons in all. Their circumstances were comfortable, and the house was clean and airy. On the 22d or 23d December 1872, one of the daughters, aged 18, and the grandchild, aged 6 years, took ill, and both went through an attack of typhus, for which they were treated at home.

About the beginning of January 1873, the other daughter sickened, had well-marked typhus, and recovered. She also was treated at home. Then the son-in-law took ill, and died. He also was treated at home. About the same time Mrs Stewart was seized and also died. She too was treated at home. Mr Stewart was then seized. There being no one at home fit to attend to him, he was removed to the Infirmary, where he died about ten days after his wife's death.

The only member of the family who escaped was the son, who took fright, left his father's house, and went to a lodging about the time that his second sister was seized.

Mrs Stewart had a married daughter, who, with her husband (James Ogilvie), and one child, aged eight months, resided in

William Street, at least a mile from the Stewarts' house. James Ogilvie shivered on the 27th December 1872, and died of typhus in his own house the second week of January 1873.

Ten days after her husband's death, Mrs Ogilvie went to the neighbourhood of Arbroath. About a week after her arrival there she took ill, had a well-marked attack of typhus, and recovered. The child remained well.

(*c.*) At a different part of the same block of buildings resided another family of the same name as, but not in any way connected with, the last. To prevent confusion, I shall call them Smart. The family consisted of Charles Smart, his wife, and two boys—Frank, aged 7, and William, aged 13 years. In December 1872, Frank had what was said by the friends to have been "gastric" fever, for which he was treated at home, and of which he died on the 24th December. The medical man who attended him certified that he died of tabes mesenterica. His father and brother both shivered about the 8th January 1873, and on the 15th were both removed to the Infirmary, where they both died of undoubted typhus—the father on the 17th and the son on the 19th January. Mrs Smart remained well; she had had "fever" some years ago.

Other cases afterwards appeared in Dudhope Crescent, but these were all the early ones.

II. *Mid Road.*—(*a.*) In Jolly's land in this road resided a labourer named Michael Carr. His household consisted of himself, wife, and five children, aged respectively 8, 10, 12, 14, and 17 years.

About the middle of November 1872, William Carr, aged 8 years, took ill, and went through an attack of "gastric fever," for which he was treated at home, and from which he recovered.

About the end of November, his sister Margaret, aged 12 years, was seized with the same illness, was also treated at home, and also recovered.

About the middle of December, the father, aged 46, the mother, aged 44, a son, John, aged 14, and a son, Michael, aged 10, all took ill, and were removed to the Infirmary, suffering from undoubted typhus fever. The father and children recovered, but Mrs Carr died.

Thomas Carr, aged 17, the only remaining member of the family, was seized about the 25th of January 1873, and was removed to the Infirmary, where he too went through a well-marked attack of typhus, and recovered.

(*b.*) At Mackenzie's land, in the Mid Road, close to where the Carrs resided, there lived a man named James Lowden, aged 60, his wife, aged 40, and a son, aged 9 years.

The father shivered on the 12th or 13th February 1873, and was removed to the Infirmary, where he died of typhus on 24th February.

His son took ill about the same time, and was also removed to the Infirmary, where he went through a well-marked attack of typhus, and recovered.

Mrs Lowden remained well. She had formerly suffered from fever of some kind, and on this occasion took care to expose herself as little as possible to the risk of catching it again.

Lawrence Street.—At No. 17 resided Mr and Mrs Bruce, and their daughter Elizabeth, a girl of 17 years: this last shivered on 20th December, and was admitted to the Infirmary on the 28th with the typhus eruption out. She recovered. No other case occurred among her friends or in her neighbourhood.

Such is the history of the earliest cases of the outbreak, and of the appearance of the disease in the localities first affected. Whilst it was so prevalent in Dudhope Crescent and Mid Road there could be found no evidence of its existence in other parts of the town. Soon, however, it began to be heard of in other quarters; the localities next invaded being the Murraygate, Overgate, Cowgate, and adjoining streets.

Its occurrence in these localities was clearly traceable to infection derived from Dudhope Crescent and Mid Road, and from other quarters, as the following facts will show:—

The first case which occurred in any other part of the town was that of Mrs Coutie, already referred to, who nursed Mrs Smith, who died in Dudhope Crescent on 24th December. Mrs Coutie died of typhus at 65 Murraygate early in January. The disease did not spread to others from her.

Two families were mainly instrumental in importing the disease to other parts of the town—the Smarts of Dudhope Crescent, and the Lowdens of Mid Road.

Mrs Smart had two sisters resident in Dundee—Mrs Gloag, residing at No. 112 Overgate, and Mrs Walker, residing at No. 16 Murraygate.

As already noted, Frank Smart died at home on 24th December. On the evening of that day, his father, mother, and brother went to the house of Mrs Gloag, where they remained till the 1st January, when they returned home. About the 8th January, Mr Smart and the remaining son took ill; they were removed to the Infirmary on the 15th, where, as we have seen, they both died of typhus. On the same day on which they went to the Infirmary, Mrs Smart returned to Mrs Gloag's, where she remained till the 1st March.

The Gloag family consisted of Mr and Mrs G. and four children. Mr G. is employed in a coasting steamer, and is at home every week from Friday till Tuesday. On the 11th or 12th February he was seized with the premonitory symptoms of what turned out to be typhus. He was treated at home, and recovered. No other case occurred in the house, but Mrs Gloag and the children all had typhus three years ago. This was the first case which occurred in the Overgate.

Mrs Smart's other sister, Mrs Walker, residing at 16 Murraygate, was in frequent communication with Mrs Smart during the illness of her husband and children, and on the 19th January assisted to dress the bodies of Mr Smart and the son, who died in the Infirmary.

The Walker family consisted of Mr and Mrs Walker and three sons. On the 11th or 12th February, the husband, William Walker, aged 60, took ill, and was removed to the Infirmary, where he died of typhus on the 25th February. His two boys, David, aged 17, and George, aged 12, who were admitted at the same time, also had typhus, but recovered. The remaining son, Charles, aged 8 years, shivered on the 17th March, and was admitted to the Infirmary on the 26th with the typhus eruption out. Mrs Walker remained well.

Thus the appearance of the disease in the Murraygate was clearly traceable to its previous occurrence in Dudhope Crescent.

But this was not the only source of infection for that locality.

It has already been noted that James Lowden, aged 60, and his son, aged 9 years, residing at Mid Road, both undoubted cases of typhus, were admitted to the Infirmary on the 17th February. The father died in the Infirmary on the 24th February.

Mrs Lowden had a sister, Mrs Ross, who, with her husband and four children, resided at 10 Murraygate. For a week after her husband's death, Mrs Lowden resided with Mrs Ross. On going to her house, she took with her some of the bedclothes which had been used by her husband and son before their removal to the Infirmary.

Mrs Lowden came to her sister's house on the 25th of February. On the 10th of March, Mrs Ross and two of her children, David, aged 14, and Eliza, aged 10 years, took ill, and were admitted to the Infirmary on the 17th, suffering from undoubted typhus. A week later, Mary Anne, aged 9, was also admitted, similarly affected. The father, and a child aged 2 years, escaped. The former had "spotted fever" twenty-three years ago.

At 133 Overgate, nearly opposite the Gloags', resided a widow named Mrs Duncan; a son, John, aged 17; a son, Alexander, aged 7; and a young woman, Jane Chalmers, aged 20. Their accommodation consisted of one room. They came from Aberdeen to Dundee at the end of February or beginning of March. Mrs Duncan stated that fever was prevalent in that part of Aberdeen from which they came, and I learned from Dr Paterson of that city that the form of fever was typhus. Mrs Duncan's youngest son was ailing for a few days before they left Aberdeen, and for three weeks after coming to Dundee was confined to bed with "gastric fever." Before he had thoroughly recovered, his mother and his brother John sickened, and, on the 13th and 15th A respectively, were removed to the Infirmary, where they were both found to be suffering from typhus.

Jane Chalmers remained well. She had "fever" some years ago.

After the removal of the Duncans to the Infirmary, a girl named Mary Anne Barclay went to reside with Jane Chalmers in the Duncans' house. She went to the house for the first time on 19th April. On the 3d of May she shivered, and on the 6th was removed to the Infirmary, where she went through typhus.

Jane Chalmers had a sister, Isabella, who resided with Mrs Connolly at 121 Cowgate. Isabella was in the habit of going to see her sister Jane at 133 Overgate after the removal of the Duncans to the Infirmary.

The household of Mrs Connolly, with whom she resided, consisted of Mrs Connolly, her husband, three boys, aged 5, 10, and 12, a girl aged 7, and Isabella Chalmers. James Connolly, aged 10, took ill on the 26th April; a day or two later John, aged 12, was similarly seized; two days later, Isabella Chalmers shivered; she and John were removed to the Infirmary, where both were found to be suffering from typhus, and where John died on the 12th May. James was treated at home, and recovered. The girl aged 7, and the remaining son aged 5, were next seized, were treated at home, and recovered. A day or two after the removal of Isabella Chalmers and John Connolly to the Infirmary, Agnes Ross, aged 18, took up her abode in the house. On the 26th May she shivered, and on the 2d June was admitted to the Infirmary with the typhus rash on her skin. Mr and Mrs Connolly had spotted fever in Ireland a number of years ago.

These were the earliest cases which occurred in the Cowgate. The next cases occurred in the family of a Mrs Reid living on the opposite side of the street, to whom the disease was no doubt conveyed by the Connollys, the young members of the two families being in the habit of mixing a good deal together. Within the next few weeks cases occurred in various houses in this street.

Such is the history of the first cases which occurred in the Murraygate, Overgate, and Cowgate. Once introduced to these localities, the disease soon spread in them, and from them to adjoining streets.

Thus, from the Murraygate it was introduced to the Wellgate, Seagate, and High Street; from the Overgate to Temple Lane, Blackness Road, and Scouringburn; and from the Cowgate to Blackscroft and Seagate. To give the full details of its mode of introduction to each new locality would, with only an alteration of the names, be a repetition of such facts as have already been recorded regarding its mode of introduction to the Murraygate, Overgate, and Cowgate.

Not every case, however, could thus be clearly traced to communication (direct or indirect) with a pre-existing one. Several sporadic and solitary cases there were of whose causation no explanation could be given. The case of Elizabeth Bruce in Lawrence Street is a case in point. Two others may be given by way of illustration:—

Thomas Jackson, from Bell Street Lane, was admitted on 9th January 1873, with the typhus eruption out. No explanation of the occurrence of this case could be got, and no others existed in the neighbourhood before or after.

Alexander Thomson, aged 18, resided at 7 Rosebank Road with his mother and sister. He shivered on the 30th January, and was sent to the Infirmary on the 8th February 1873, where he went through a well-marked attack of typhus.

No reason for the occurrence of this case could be found. It seemed quite an isolated one.

The above facts suffice to show that, though there occurred some cases of whose mode of origin no explanation could be given, the mass of the cases which occurred throughout the town could clearly be traced back to infection derived from a previously existing case.

Had the disease not previously existed in Aberdeen, it probably would not have occurred at 133 Overgate. Had there been no fever in Dudhope Crescent or Mid Road, it would not, so far as we can see, have had any existence in the Murraygate; and had there not been direct communication between 121 Cowgate and 133 Overgate, there is no reason why the former street should have been invaded. Had the disease been kept out of these localities, there is no reason why it should have been heard of in those other parts of the town in which it prevailed for several months. There is abundant and direct evidence that the vast majority of the cases which occurred in 1873 were clearly due to infection derived from previously existing cases. A fair sample of this evidence I have given in the details of the earlier cases which occurred in that year.

But what of those which occurred in December 1872?

We have seen that for a period of six months subsequent to the 17th June 1872, no case of typhus was admitted to the Infirmary, and that in the fourth week of December no fewer than thirteen cases came from three different parts of the town. Now, taking the bare facts of the re-appearance of the disease as thus stated—finding it thus breaking out at the same time in three different and widely separated parts of the town, and that too after so long an absence—it would be natural to attribute its re-appearance to the existence of some epidemic influence, some general all-pervading cause which, whatever its nature, cannot be controlled or have its operation stayed by human means. Such a supposition would be strengthened by the fact, that in a very short time the disease appeared in other localities separated by a considerable distance from those first affected, and in the course of a few months had made its presence felt in every district of the town.

The latter part of the supposition, however, we have seen to be quite unnecessary. It has been shown that the disease, as it existed in the early months of 1873, was the result of its introduction to fresh localities by those already affected, or by their friends; and that it was, therefore, due to an agency capable of being influenced or even controlled by human means.

It shall be my endeavour to show that even the first cases—those which occurred in December 1872, after typhus had apparently been absent from the town for six months—were not of spontaneous origin. I say, had *apparently* been absent, for it shall be one of the points of my argument that, though no cases appear in the hospital records between June and December 1872, the disease, nevertheless, was not entirely absent from the town.

The localities in which the disease first made itself known in December 1872 were Dudhope Crescent, Mid Road, and Lawrence Street. We shall examine separately the circumstances under which it appeared in each.

Dudhope Crescent.—The first thing to be noted regarding the outbreak in this locality is that the cases sent to the Infirmary do not accurately represent either the time at which the disease first appeared, or the full extent of its prevalence. The first cases were sent to the Infirmary on the 20th December, but at least two cases had occurred before that time—that of Mrs Smith, who died on the 4th, and that of Frank Smart, who died on the 24th December. The number of cases sent to the Infirmary from Dudhope Crescent at the early part of the outbreak was twelve; the number which occurred in the Crescent was nineteen: seven were treated at home. The first brush of the outbreak in this part, then, consisted of nineteen cases. Of these, eighteen were furnished by three families—the Smiths, nine; the Stewarts, six; and the Smarts, three. The remaining case was that of Mrs Aitken, who helped to nurse Mrs Smith. How was it introduced to each family? The first case was that of Mrs Smith, who died on 4th December 1872. Supposing she died on the fourteenth day of the fever, her ailment must have commenced on or about the 20th November.

Frank Smart died on the 24th December: his illness probably commenced about the 10th.

The first of the Stewarts affected shivered about the 22d December.

Now, it is to be noted that there was no personal communication between any two of these families, and that no reason could be found for the occurrence of the first case in each. But though there was no communication, it cannot positively be said that there was no contact of the members of one family with those of either of the others. Mrs Smith shivered about the 20th November, the first of the Smarts about the 10th of December, and the first of the Stewarts about the 22d December. Residing as they all did in the same locality, it is not unreasonable to assume that Mrs Smith was the ource whence was derived the infection which prostrated the Smarts and the Stewarts. That the occurrence of a case of typhus n a densely populated locality should be followed by others, is no more than what is to be expected; and, in the absence of evidence of any other source of infection, the first case is justly regarded as the source of all the others, even though there may have been no direct communication between them. The prior occurrence of Mrs Smith's case, therefore, offers a reasonable explanation of all those which subsequently occurred in Dudhope Crescent.

But, on the other hand, certain reasonable objections may be adduced against this view. If Mrs Smith were the original source of infection for all the cases which occurred, it is only natural to suppose that those with whom she was in personal contact, the members of her own family (to whom she undoubtedly communicated the disease) would be the first to suffer. Such was not the case, however, for none of them shivered before the 12th December; and so far as could be ascertained (for there is no absolute certainty in the matter), the boy Smart shivered about the 10th; at the very least, it may be said that his illness commenced as soon as that of the members of Mrs Smith's family. So far as the Smarts are concerned, then, it is doubtful whether they derived the infection from Mrs Smith, though, in the absence of proof of any other source of infection, it cannot be positively asserted that they did not do so. In the Stewarts, the disease declared itself ten days later. Springing up as it did almost simultaneously in two members of the family, it was almost certainly introduced from without; and occurring as it did ten days later than the date of its appearance in the members of Mrs Smith's family, there is not the same reason which obtained in the case of the Smarts for looking beyond Mrs Smith for the source of infection. There is another reason for doing so, however. It has already been noted that Mrs Stewart had a married daughter, Mrs Ogilvie, who, with her husband and child, resided at William Street (a mile distant), in an open airy locality, in which there was no fever. It has been further noted that the husband, James Ogilvie, shivered on 27th December, and died at home of typhus early in January. There was frequent communication between the Stewarts and Ogilvies. The fact that the first of the Stewarts seized shivered on the 22d or 23d December, and that Ogilvie shivered on the 27th, and the additional fact that he had not seen any of the Stewarts for a week previous, preclude the idea that the one got the infection from the other, and render it all but certain that both derived it from the same source. Now, what was this source? Was it Mrs Smith? or was it some other? The only places at which there was direct personal communication between the Stewarts and Ogilvies were the Stewarts' house in Dudhope Crescent, and the church, at which they both sat in the same pew. No evidence could be found of their being in any way exposed to infection in church: none of those who sat near them had fever in their household. We are therefore thrown back on the only other possible source of a common infection, the Stewarts' house, in the immediate neighbourhood of which typhus fever existed prior to, and at the time of, its appearance in their family. The conclusion, therefore, is forced upon us, that both the Stewarts and James Ogilvie got the poison in Dudhope Crescent, and from the same source; the only houses there in which it existed at that time were those of the Smiths and the Smarts.

It follows from this that Frank Smart's case finds in Mrs Smith's a possible but not altogether satisfactory explanation of its occurrence, and that the same case offers a probable but not certain solution of those which subsequently occurred among the Stewarts and Ogilvies. If we reject infection derived from Mrs Smith as the cause of these cases, there remains, besides spontaneous generation, but one other cause to which their occurrence can be attributed, viz., that to which Mrs Smith's attack is to be ascribed. Her case was admittedly the first which occurred in Dudhope Crescent, and may be the source whence all the others derived their infection; but, on the other hand, it is quite possible that these others may have been due to the same cause which gave rise to Mrs Smith's. What was that cause? Did the case arise spontaneously? or was it due to infection derived from a pre-existing one? For argument's sake, we will at once admit that the disease may originate *de novo.* The circumstances which lead to its occurrence are overcrowding, bodily privation, mental depression, exposure, and the various miseries which these entail. Now, none of these conditions obtained in either the Smiths', the Smarts', or the Stewarts'. In the Smiths' alone was there less accommodation than was desirable, but even in their case there was not what could be called overcrowding. Their house consisted of two good-sized rooms. As a rule, no one was at home during the day except Mrs Smith and the two youngest children, aged 5 and 9 years. Mrs Smith was a hard-working, industrious woman, who attended well to her family, and kept the house clean and comfortable. The father and four of the family were in receipt of good wages: they were well fed and well clothed, and indeed suffered from no privation. The Smarts were equally well off, and had ample breathing space. The Stewarts were in still better circumstances: they were quite superior in every way to most of their neighbours, were evidently in very comfortable circumstances, and had a well-furnished commodious house.

It is evident that the circumstances of those first attacked were not such as to give rise to those conditions which lead to the production of typhus. There were scores of families round about in whom these conditions could more readily have been produced, but who escaped the fever. Beyond the fact, then, that no cases of the disease were known to exist in the town for six months previously, and that no evidence of importation could be found, there are no grounds for regarding these cases as of spontaneous origin.

Not being of spontaneous origin, they were of course due to infection derived from a pre-existing case. Now, what evidence is there of the existence of such a case? Of direct evidence, none; but of indirect, not a little.

It has been seen that between the 17th June and the 20th December 1872 not one case of typhus was admitted to the Infirmary. As a rule, typhus is treated in hospital. The admissions to the Infirmary, therefore, form the best available indication of the extent of its prevalence at a given time. Two circumstances, however, intervene to render this an uncertain and fallacious (even though the best available) index of the extent to which the malady prevails. In the first place, many cases known to be typhus are treated at home, and are never sent to hospital; and in the second place, many cases occur in which the true nature of the ailment is not recognised. We shall by-and-by see that in the summer of 1872 there were special agencies at work calculated to render both these circumstances more than usually operative. Meantime we must give some proof of their actual existence.

1. *Many cases of typhus are treated at home.*

Even in the few cases which I have given there is abundant evidence of this. Of the six members of the Stewart family who suffered, all of whom were known to have typhus, five were treated at home. James Ogilvie, too, likewise known to have typhus, died in his own house. Mr Gloag also was known to be suffering from it, and was treated at home. Many other instances came under my notice during the course of my investigations; but those given suffice to show that by no means all the cases of typhus which occur are sent to the Infirmary.

2. *Many cases of typhus occur in which the true nature of the ailment is not recognised.*

One would be disposed to think that a disease having such distinctive symptoms as typhus has would not readily be mistaken for others, and conversely that others would not readily be mistaken for it. That such an error in diagnosis is frequently made, is a fact well known to those who have had charge of a hospital into which fever cases are received. Patients labouring under all sorts of ailments are constantly sent in as typhus cases, and certified to be such by competent medical men. In many, probably in the majority, of such cases, there is so much difficulty, in consequence of the filth and gloom of their residence, in forming a satisfactory diagnosis that the best thing for the medical man to do is to err on the side of safety, treat the case as one of fever, and send it to hospital. Such cases are seldom seen more than once or twice at their own homes, and the diagnosis (if such it can be called) is formed after a single and unsatisfactory visit. Faulty diagnosis, under such circumstances, must be frequent, even with the best and most experienced.

But it is not with such cases that we are now dealing; their true nature is soon recognised in hospital. The fact to which I wish to call attention is that cases of typhus occur, and *are treated to a termination*, in their own homes, without the true nature of the ailment having ever been recognised. For evidence of this we need go no further than the details given in this paper:—

1. Mrs Smith, who died in Dudhope Crescent of what subsequent events proved to be typhus, was certified to have died of delirium tremens.

2. Frank Smart, who died at home on 24th December of what subsequent events proved to be typhus, was certified to have died of tabes mesenterica.

3. The first of the Duncans at 133 Overgate who sickened, and who was the original source of the numerous cases of typhus which occurred at that number, was treated at home for what was said to be "gastric" fever.

4. William Carr, aged 8, residing at Mid Road, was in November 1872 treated at home for an attack of "gastric" fever, from which he recovered.

5. His sister, Margaret Carr, aged 12, was seized a fortnight later, and was also treated at home for what was said to be "gastric" fever. A fortnight later, all the other members of the family were admitted to the Infirmary with the typhus rash out.

Thus we see that in each locality in which the disease was prevalent at the early part of the outbreak—Dudhope Crescent, Mid Road, and Overgate—the first cases did not have their true nature recognised: as a necessary consequence of such an error, there was an entire absence of all precautionary measures against the spread of the disease, and an almost inevitable extension of it to those who came in contact with the earliest sufferers.

Furthermore, it is to be noted that in each locality the true nature of the fever was not recognised till the disease simultaneously attacked several members of a household. In the instances given, the prostration at one time of so many members of the family could not fail to show that they were suffering from some infectious fever, and lead to a more careful examination than was accorded to the first cases, and to a more accurate diagnosis.

If typhus be mistaken for any other disease, it is most likely to be mistaken for that which most resembles it. We accordingly find that of the five cases of mistaken diagnosis which have been given, in three the patient was thought to be suffering from gastric or typhoid fever.

The facts of the case are as follows:—Typhus fever disappeared from the hospital records for six months, from June to December 1872. It is presumed also to have been absent from the town; at least no cases are known to have occurred. After being so long absent it again made its appearance, and a number of cases were almost simultaneously admitted from different and widely-separated parts of the town. On inquiry, it was found that the cases sent to the Infirmary were not the first which occurred, but that in each locality there had been one or more prior cases, whose true nature had not been recognised. Now, the cases in which an error in diagnosis was made cannot all have been mild cases, for two out of the five died—one (according to the registrars' certificates) of delirium tremens, the other of tabes mesenterica. Placed under more favourable circumstances, such a mistake would not have occurred. What we have to deal with, however, is not the wherefore, but the fact that such an error was made—that the first two cases of typhus which occurred at Mid Road were said to be gastric fever; that of the first two cases which occurred in Dudhope Crescent (both of which proved fatal), one was certified to have died of delirium tremens, the other of tabes mesenterica; and that the earliest case at 133 Overgate (introduced from Aberdeen) was also said to be suffering from gastric fever. Well, if so many errors in diagnosis were made in the course of one month—if five cases of typhus, attended by four different and qualified medical men, were in that time mistaken for other ailments—it is but reasonable to suppose that during the previous five months a good many similar mistakes may have been made in a town of 120,000 inhabitants, in which typhus is endemic. Indeed, we can scarcely avoid the conclusion that during that time such errors in diagnosis *must* have been made, that during the summer of 1872 cases of typhus must have occurred, and that its absence from the Infirmary records is no proof of its absence from the town.

The investigation which led to the discovery of these early and mistaken cases was not commenced till the fourth week of December 1872. It could not, therefore, be expected to throw light on the origin of sporadic cases which may have occurred two or three months previously. It did, however, lead to the discovery of five cases in which the true nature of the disease had not been recognised, and all of which had occurred during the four or five weeks immediately preceding the date of the recognition of the first cases of typhus. Considering the fact that, if typhus existed at all during the summer of 1872, it must have existed to only a slight extent, it could scarcely be expected that an investigation begun at the end of December could throw light on unrecognised cases which occurred more than a few weeks previously. It is not, therefore, to be wondered at that no evidence was got tending to show the existence of the disease during the course of the summer. The fact, however, that this investigation led to the discovery of five cases of earlier occurrence than the first cases sent to the Infirmary, and the further fact that the true nature of these cases was not recognised, afford strong presumptive evidence that these five were not the only cases which occurred between the 17th June and the 20th December, the date of the re-appearance of the disease in the Infirmary. The probability is, that cases occurred sporadically during the whole summer, and that the five mistaken cases which occurred in November and December were the lineal descendants of other similarly mistaken cases which had occurred in July, August, September, and October.

On this view of the matter, the cases constituting the outbreak which occurred in December 1872 are to be regarded not as of spontaneous origin, but as an accidental increase of an ailment which had existed to a slight and altogether unrecognised extent during the previous summer.

That cases of typhus fever may occur, and be treated to a termination, and even to a fatal termination, without the true nature of the ailment being recognised, is no mere assumption, but a fact which is proved to demonstration by the details already given. The likelihood of such unrecognised cases having occurred is rendered still more probable by the fact that during the months in which typhus has no place in the hospital records certain agencies were at work calculated to mask its occurrence, to render errors in diagnosis more probable, and to make the hospital records a more than usually fallacious test of the extent to which the disease prevailed.

These agencies were the great and unusual prevalence of relapsing fever and of smallpox. During the year 1871 these two diseases existed in an epidemic form; they ran their course side by side, each epidemic reaching its height in December 1871. So great was the pressure on the space at disposal in the Infirmary, that for two months (December 1871 and January 1872) it was found necessary to refuse admission to cases of smallpox. In February, however, relapsing fever had so abated, that smallpox cases were again received.

The following table shows the number of cases of relapsing fever, variola, typhus, enteric fever, and febricula, admitted during each month of 1872:—

Month.	Relapsing.	Variola.	Typhus.	Enteric.	Febricula.
January,........	48	1	14	1	4
February,......	25	17	12	0	3
March,..........	32	6	5	1	1
April,...........	15	7	4	0	1
May,.............	9	6	9	0	0
June,............	2	0	2	0	0
July,.......... ..	11	0	0	7	1
August,.........	14	2	0	3	2
September,.....	8	6	0	4	2
October,........	2	4	0	1	4
November,.....	0	7	0	0	2
December,.. ...	0	1	13	3	0
Total,.......	166	57	59	20	20

This table represents (so far as the Infirmary records can) the xtent to which each of the maladies mentioned therein prevailed in .872. We have already seen that these records give an inadequate dea of the number of cases of typhus which may have occurred at given time. This for two reasons—First, because not a few ases, known to be typhus, are treated at home; and second, beause in some cases of typhus the true nature of the ailment is not ecognised. Now, the prevalence in an epidemic form of smallpox and of relapsing fever was calculated to render both these agencies more than usually operative.

The report of the Dundee Infirmary for 1871–72 (June to June of each year) states, that "throughout the past year there has been a marked diminution of surgical and medical cases as compared with former years. This is to be accounted for, not by the absence or decrease of disease in our community; but a fear of contracting smallpox in the hospital deterred many from seeking admission." (*Verbatim.*)

That the report is correct in attributing the decrease of general cases to the dread of catching smallpox there can be no doubt. That this fear was a well-grounded one there is as little doubt; for the same report says, "A few cases (of smallpox) broke out in the surgical and medical wards, and in some instances proved fatal." So great was the dread of the Infirmary at that time, that even those suffering from serious accidents could not always be persuaded to avail themselves of the undoubted advantages which such an institution afforded for their proper treatment. This dread of catching smallpox was of course not confined to those suffering from general ailments, but would also operate in preventing those suffering from typhus from being sent to the Infirmary. The reception of so many cases of smallpox into hospital would therefore tend to increase the proportion of cases of typhus treated at home; and though the number of cases of smallpox admitted during the months that typhus was absent was comparatively small, it was quite sufficient to keep alive that fear of catching so loathsome a malady which we have seen to have been instrumental in keeping down the number of general cases.

These considerations render it not improbable that during the months when typhus was absent from the Infirmary records, a few cases known to be typhus may have been treated in their own homes.

The occurrence of relapsing fever in an epidemic form would operate in a different but not less efficacious manner in rendering the Infirmary records an inaccurate index of the full extent to which typhus prevailed. Smallpox would tend to increase the proportion of cases treated at home, as just explained. Relapsing fever would tend to increase the proportion in which the real nature of the disease was not recognised.

We have already seen that in November and December 1872, after relapsing fever had disappeared from the town, five cases of what subsequent events proved to be typhus were treated in their own homes. Of these, one was certified to have died of delirium tremens, and one of tabes mesenterica; the other three were said to have been cases of gastric fever. Now, if typhus can be mistaken for such different ailments as these, it certainly may also be mistaken for relapsing fever. It may, indeed, be said that the peculiar and characteristic course of relapsing fever would render such a mistake improbable; that, on the one hand, the period of intermission of the febrile symptoms, and the subsequent recurrence of these, could not fail to declare the real nature of the ailment, and prevent its being mistaken for typhus; and that, on the other, the absence of such intermission and recurrence could not fail to prevent any other fever being mistaken for relapsing. This is no doubt true of cases which are attended and watched to a termination, but in very many cases such care is not bestowed. Among the poorer classes in Dundee, there exists a practice of sending for a medical man when they are ill, and paying him for his visit at the time, the medical man not going back to see the patient again unless he is sent for, or unless the case is so grave as to make it prudent for him to continue his attendance. Well, suppose, under such circumstances, a medical man is sent for to see a boy who has been ailing for some days. He finds a lad suffering from febrile symptoms, and no local lesion to account for them; he tells the parents that the boy has got "fever," and probably recommends his removal to the Infirmary. This they are unwilling or even refuse to do, partly in consequence of his mother's wish that he should remain at home, and partly from dread of his catching smallpox in the Infirmary; at any rate, he is not sent there. The medical man, in the daily habit of seeing numerous cases of relapsing fever, and knowing that that form of fever is the only one prevalent at the time, naturally enough thinks that such will be the nature of this case. He accordingly warns the parents that the fever is catching; gives such directions as he considers requisite for diminishing the risk of its spreading to other members of the family; tells them that the boy is not in danger, and that he will soon be all right; gives a prescription for some febrifuge mixture, with a few directions regarding diet, and leaves the case. If the case be one of relapsing fever, he may again be sent for when the relapse occurs; but if it be a mild case of typhus (and typhus is often very mild in young people), he sees no more of the patient, the real nature of whose ailment is thus never recognised. Even in cases in which a more regular attendance is given, such an error in diagnosis may be made; the attendance will cease even then with the decline of the febrile symptoms; and unless the medical attendant has been careful to inquire into the exact duration of the febrile symptoms, and has been able to determine whether or not a rash exists, he may pronounce a case to be relapsing fever which was in reality typhus. One can quite understand how an overworked nd ill-paid practitioner, seeing a patient suffering from febrile symp- ms, and not in any danger, and knowing that relapsing fever was pidemic, would pronounce the case in hand to be so, without roubling to examine the skin in an insufficiently-lighted room, and ithout inquiring minutely into the duration of the symptoms. If yphus were mistaken (as we have seen it was) for delirium tremens, abes mesenterica, and gastric fever, it is most probable that, under uch circumstances as those indicated, it was sometimes mistaken for relapsing fever. The circumstances and surroundings of that lass among whom both typhus and relapsing fever are most prevalent, are such that it is often difficult, and sometimes impossible, with all the care that can be bestowed, to determine whether or not there is a rash on the skin. Those who attend such cases are generally men who have a very large number of visits to make in the course of the day, and who, therefore, cannot afford to spend more time than is absolutely necessary with each patient. Finding a patient, in the midst of an epidemic of relapsing fever, suffering from all the symptoms of that disease, he is very apt at once to put the case down as such, without wasting time in inquiries which can, he thinks, lead to no practical benefit. I have little doubt that in this manner cases of typhus were mistaken for relapsing fever, and that the prevalence of the latter in the summer of 1872 is another reason for the belief, that though typhus was absent from the Infirmary, it was not absent from the town during the months of July, August, September, and October.

On these grounds, then,—on the grounds that a certain proportion of cases of typhus are treated at home, and that cases do occur in which the true nature of the ailment is not recognised, and on the additional grounds that there existed in the summer of 1872 special reasons why these agencies should be more than usually operative,—I have come to the conclusion that typhus was not really absent from the town during the months of its disappearance from the Infirmary records, and that the cases which constituted the outbreak which took place in December 1872 were the lineal descendants of those which occurred during the early months of that year.

That sporadic cases occurred in December 1872 and January 1873 is evident from the details given. Witness the cases of Elizabeth Bruce, Lawrence Street; Thomas Jackson, Bell Street Lane; and Alexander Thomson, Rosebank Road (all widely separated localities), for the occurrence of no one of which could the semblance of a reason be found.

The first case which occurred in Dudhope Crescent (that of Mrs Smith), and the first in Mid Road, were also sporadic. The outbreak of the disease to which each gave rise in its own locality was due to the accidental circumstance that each happened to be a member of a large family, and that the season at which they occurred was that at which every effort is made to exclude the cold external air; and at which, therefore, that condition of atmosphere which is peculiarly adapted for the spread and propagation of typhus fever most obtains in the dwellings of the lower classes.

The decline and ultimate disappearance of relapsing fever from the town at the end of 1872 removed one probably great source of errors in diagnosis, and so favoured the recognition of the true nature of any cases of typhus fever which occurred, such as the sporadic ones already referred to; while the profession, as well as the public, had its attention drawn to the existence of typhus in the town by a paragraph in the local newspapers, intimating the admission of so many cases to the Infirmary. This, too, no doubt, tended to diminish the risk of errors in diagnosis.

We have seen that a combination of circumstances rendered the Infirmary records an unusually unreliable test of the extent to which typhus prevailed in the town in the summer of 1872. We have also seen that the most potent of these agencies—the prevalence in an epidemic form of relapsing fever and variola—had ceased to operate by the end of that year, at which time there also came into play an agency peculiarly favourable to the propagation of typhus, viz., deficient ventilation.

To form a true idea as to the circumstances which gave rise to the outbreak whose causation we have been investigating, we must take into consideration each of the various circumstances and agencies to which reference has been made, and give to each its due weight. This I have endeavoured to do fairly and impartially, and have been forced to the conclusion that the outbreak of typhus which took place in December 1872 was not so abrupt as a superficial investigation would lead one to suppose; that there is very strong evidence that the disease did not originate *de novo;* and that it had never ceased to exist in its usual endemic form.

Some very important practical deductions are suggested by such a consideration as has been given to the early cases of the outbreak. The chief of these are the importance of an accurate diagnosis, the importance of early removal to hospital, and the importance of proper disinfection of the bedclothes, etc. Had the case of Mrs Smith, who died in Dudhope Crescent on 4th December 1872, been properly diagnosed, she removed to the Infirmary, and proper means at once taken to have the house and all in it properly cleaned, it is not at all improbable—certainly it is quite possible—that not another case would have occurred there. Of the benefit which might have accrued to Mrs Smith if the true nature of her ailment had been recognised, it is unnecessary to say anything. What we have to consider is the benefit that might, and not improbably would, have resulted to others from her early removal to the Infirmary.

The cases that, as we have seen, may be traced back to the previous occurrence of the disease in Mrs Smith, are (*a*) the other members of her own family, eight in number, who all recovered; (*b*) three Smarts, who all died; (*c*) six Stewarts, of whom three recovered and three died; (*d*) Mrs Aitken, who recovered; (*e*) Mrs

Coutie, who died; (*f*) Mr Gloag, who got the disease from the Smarts, and recovered; (*g*) the Walkers, who got it from the same source, and of whom three recovered and one died; (*h*) Mr and Mrs Ogilvie (son-in-law and daughter of Mrs Stewart), of whom one recovered and one died. Thus we have no fewer than twenty-seven cases, all directly traceable to the previous occurrence of Mrs Smith's case. Now, I do not think it is too much to say, that the majority, if not the whole, of these cases might have been prevented if the true nature of her ailment had been recognised, and she removed to the Infirmary. But to say, merely, that twenty-seven cases of typhus might have been prevented, is to give a very inadequate idea of the extent of the mischief done. Not only did twenty-seven preventable attacks of typhus occur, but, sadder and more painful still, nine valuable lives were lost which might otherwise have been spared. Mrs Smart, previously the head of a happy household, lost her husband and both her boys, and thus, at once, became a widow and childless. The family of the Stewarts, a pattern of united love and happiness, was completely broken up; the father, mother, two sons-in-law—all cut down, and the girls compelled to break up their home and earn their bread as best they could; and this, too, when they themselves were wasted and worn by disease. In this family alone there were thus created one widow and six orphans. Mrs Walker lost the husband who was the support of herself and family.

Again, if the case of the boy Duncan, from Aberdeen, had been recognised as one of typhus, and he sent to the Infirmary, the numerous cases in the Overgate and Cowgate, which could clearly be traced back to him, might have been prevented.

And so, too, with the outbreak at Mid Road; if the true nature of the illness of the first of the Carrs who suffered (about the middle of November) had been recognised, and proper measures taken to prevent the spread of the disease, there is very little doubt that it would have gone no further. Mrs Carr would have been spared to her young family. James Lowden would not have died. The disease would not have been introduced by Mrs Lowden to the Rosses' house in the Murraygate, and many cases would have been prevented in that locality.

To sum up: if the true nature of the illness of Mrs Smith, in Dudhope Crescent, of the boy Duncan, at 133 Overgate, and of the first of the Carrs, at Mid Road, had been recognised, and proper measures taken to prevent the spread of the disease, there can be little doubt that most of the cases which occurred in 1873 would never have existed, and that nothing more than the usual number of endemic sporadic cases would have been heard of. Many valuable lives would have been spared, and much suffering and distress warded off.

Hitherto we have referred chiefly to the evils resulting from faulty diagnosis. These are undoubted, but they are also, to some extent, unavoidable. The gloom and filth with which the patient is often surrounded, his inability to obtain proper attendance, and the too frequently over-taxed time and energies of the medical man who sees him, all conspire to render a mistake in diagnosis by no means unlikely. The first-mentioned circumstances often render it very hard to diagnose typhus, even with all the care that one can bestow.

But there are other and preventable causes of the spread of typhus. Chief among these is the practice of treating cases in their own houses. Of the often calamitous results of such a practice, the sad havoc wrought in the Stewart family is a telling example. There was no mistake in diagnosis in this instance. Even the earliest cases were known to be typhus: they were nevertheless treated at home, with the result that three most valuable lives were sacrificed, which might have been saved by the early removal to the Infirmary of the first cases. The case of the Stewarts is by no means a solitary one.

Of the propriety of removing to hospital every case of typhus which occurs among the labouring classes, there can be no doubt: and it should be a part of our professional creed to put very clearly before one in whose house the disease makes its appearance, and in which the patient cannot be isolated, the great risk that there is of its spreading to others in the house and neighbourhood; and to urge them, by every argument in our power, to sanction the removal of the patient to the Infirmary. That there should be on the part of the friends a desire to retain the sick one at home; that the mother should be unwilling to delegate to others the nursing of her son, or the wife the care of her husband, is most natural; but if the advantage to the patient of skilled nursing and constant medical attendance, and the risk run by other members of the household, be duly and properly placed before them, I think the cases are few in which much opposition would be made to the removal of the sufferer.

Of the danger of treating typhus at home, an instance has been given in which every member of the family, except one who left the house, was prostrated, one after another. An instance of the good resulting from early removal, we have in the case of Elizabeth Bruce. She was removed to the Infirmary as soon as the nature of her ailment was recognised, and not another case occurred in the neighbourhood. If she had been treated at home, it is most probable that her father and mother would have suffered (and proprobably, seeing their age, have died), and that the disease would have spread to some of their neighbours. Her timely removal prevented all this.

But to remove the patient is not sufficient. Care should also be taken to disinfect or purify every article which may have been contaminated. That typhus may be conveyed by fomites is well known. An instance of its being so conveyed we have in the case of the Ross family, residing at 10 Murraygate, to whom the disease was brought in clothing which had been used by James Lowden and his son during their illness prior to their removal to the Infirmary, and which was brought to the Rosses' house by Mrs Lowden.

Yet another fact the above details serve to demonstrate; that is, the poison may be conveyed from one person to another by means of a third party who himself escapes. Of course, in this case too, the poison is conveyed by fomites, the clothes of the third party. Of this two instances have been given. At 16 Murraygate, William Walker and his three children were seized; to them the poison was conveyed either by Mrs Walker (sister of Mrs Smart), who dressed the bodies of the Smarts in the Infirmary, or by Mrs Smart on some of her occasional visits: neither Mrs Walker nor Mrs Smart, however, took the fever. Isabella Chalmers, there can be no doubt, was the means by which typhus was conveyed from the Duncans' house in the Overgate, in which her sister resided, to that of Mrs Connolly in the Cowgate, in which Isabella herself resided. She was in the habit of going from one house to the other, and was herself seized with the early symptoms of typhus at the end of April. But it is to be noted, that John and James Connolly were both attacked two or three days before Isabella Chalmers began to complain: the probability is, either that they imbibed the poison which had been temporarily attached to her clothing, and before she herself had inhaled any of it, or that the delay in the occurrence of her attack was due to a longer period of incubation. Certainit is, that her person was the medium by which the poison was conveyed from the Duncans to the Connollys.

The above details show the great importance, (1) of accurate diagnosis; (2) of early removal of the case to hospital; (3) of properly purifying and disinfecting the patient's clothes, etc.; (4) of temporary isolation of those resident with him. The third and fourth points are the most difficult of attainment; but they are not unattainable. Proper attention to the first and second, however, would greatly diminish any risk arising from mal-observance of the third and fourth, while a rigid observance of all would certainly tend to check the spread of typhus in a community, and might even nip a commencing epidemic in the bud. The first two points are undoubtedly the most important, as without due attention being paid to them, the third and fourth are unattainable. Of the evil effects resulting from their non-observance, we have sufficient evidence in the details given.

One other point this investigation demonstrates, viz., that the sudden re-appearance of typhus in a community, even after an absence of six months, is no proof that the disease has been developed *de novo*. This is not the least important of the lessons taught by the outbreak whose causation we have been investigating. Without careful inquiry into the circumstances of the early cases, it might have been regarded as evidence in favour of the opinion, that the typhus poison may originate *de novo;* with such inquiry, it ceases to give support to that view.

ARTICLE III.—*The Operation of Repositio Ciliorum for Trichiasis.* By D. ARGYLL ROBERTSON, M.D., F.R.C.S.E., Ophthalmic Surgeon to the Royal Infirmary, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 4th March.*)

THE operation of repositio ciliorum, also termed illaqueatio, was known to Celsus, and by him carefully described in his well-known work. Celsus himself, however, expresses an unfavourable opinion of the operation; and this appears to have been shared by his contemporaries, as this method of treating trichiasis fell into disuse, and was completely abandoned until about twelve years ago, when Dr Snellen of Utrecht re-introduced it. Notwithstanding Dr Snellen's advocacy, the operation has not been very extensively adopted, and is scarcely referred to in any of the standard works on diseases of the eye. As it is, in my opinion, an operation of great value when applied to suitable cases, I have deemed it advisable to direct again the attention of the Profession to it. The principle of the operation consists essentially in causing the offending eyelashes to be mechanically turned away from the eye, and made to grow more or less in the proper direction by making them pass under a narrow bridge of skin. The following is the method of performing the operation:—A very fine curved needle has the two extremities of a very fine waxed silk ligature (or hair, as Celsus directs) passed through its eye. The needle, being firmly grasped by suitable forceps, is then passed through a narrow fold of skin at the very margin of the lid, close to one of the inverted eyelashes. The point of introduction should be external to the point of emergence of the eyelash, but as close to it as possible; and the needle should be brought out after passing about $\frac{3}{4}'''$ or $1'''$ under the skin. The needle and ligature should be drawn through until a small loop alone remains, when, by means of a fine pair of forceps, the eyelash is passed through the loop. Traction is then made on the ligature, and the loop, with entangled eyelash, is drawn through the tunnel in the skin.

The other misdirected eyelashes are similarly treated. The chief difficulty in the performance of the operation consists in getting the eyelash entangled in the loop of the ligature, as the bleeding which occurs from the points of puncture causes the eyelashes to be matted together, or to adhere to the ligature, and the misdirected eyelash is almost always enveloped by a small clot. When this source of annoyance is troublesome, it is advisable to wait a minute or two after the passage of the needle before proceeding to the ensnaring of the eyelash. In some nervous patients, I have found it advisable to introduce the lid-forceps or clamp, so as to stretch the lid and enable it to be more readily fixed, and thus ensure accuracy in the points of introduction and emergence of the needle. One thing that must be carefully attended to is to make the point of introluction of the needle as close to the offending eyelash as possible, s, if it be made at a little distance, the resiliency of the hair causes t gradually to emerge from the tunnel and resume its former osition.

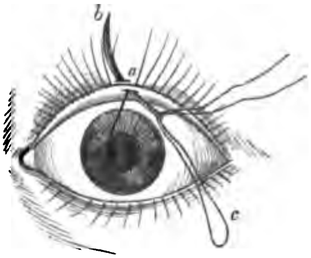

FIG. 1 represents the passage of the needle, armed with the ligature, under a band of skin close to the point of emergence of an inverted eyelash. (The direction of the needle has been represented a little oblique, not to complicate the diagram.)

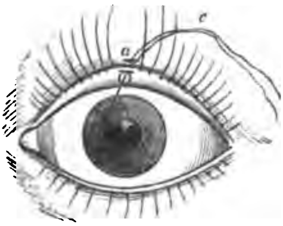

FIG. 2 represents the misdirected eyelash involved in the loop of the ligature.

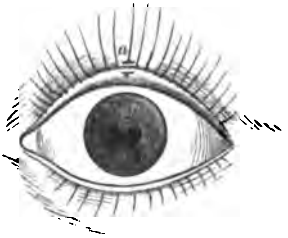

FIG. 3 represents the eyelash in its altered position.

Very little irritation follows this operation, and no special after-treatment is necessary. The patient, however, must be warned against touching or rubbing the eyelashes for twenty-four hours after the operation, as he might thus force the eyelashes back out of their new channel.

My attention was first directed to this operation by a short notice of a paper on it by Dr Schulek, in the *Annales d'Oculistique* for 1871; and, since then, I have performed it in about a dozen cases. In all it has been attended with excellent results, except the first time I employed it, when I made the mistake of using too coarse a needle, and also attempted to pass several of the eyelashes through one of the tunnels. The result was, that most of the eyelashes freed themselves from restraint, and returned to their original position. I subsequently operated on this case with success.

On examining cases that have been successfully operated on after the lapse of several months, no eyelashes are found passing under bridges or bands of skin, and yet all the eyelashes are properly directed. I have not yet been able to satisfy myself of the process by which this state of matters is attained. In some cases, particularly where the bridge of skin under which the eyelash has been passed is very thin, I believe the eyelash ulcerates its way through; in others, again, it is possible that the eyelash gradually slips back out of the tunnel, but, having been for some time kept in a proper direction, has a normal curvature given to it, and a proper direction imparted to the follicle which produces it, which direction they retain. I believe, however, that, in the majority of instances, the eyelashes after a time are shed and fall out of the tunnel, and the new ones that are generated grow in the normal plane.

This operation is, unfortunately, only applicable to those cases of partial trichiasis in which the eyelashes inverted are few in number and of considerable size. In such cases it is, I am convinced, much superior to any other method of treatment employed at the present time. Where, however, the trichiasis is general, this method of operating would be excessively tedious and ineffectual, owing to the fineness of some of the inverted lashes. Where, on the other hand, the misdirected hairs are few and fine, the operation described is inapplicable, as the difficulty of getting them entangled in the loop of the ligature is much increased; and, even if successfully accomplished, and the eyelashes drawn into the tunnel, they are very apt to free themselves, not being sufficiently firmly held in their new position.

The reason why the operation in ancient times fell into disuse, is evidently to be found in the fact that it was then employed in all forms of trichiasis; and, as suitable cases form only a comparatively small proportion of the whole, the general results of the operation could not have been satisfactory.

To Dr Snellen great credit is due for several ingenious operations upon the eyelids he has devised, as well as for the re-introduction of this old operation.

ARTICLE IV.—*Report of, and Observations upon, a Case of Twins with Double Placenta Prævia (Partial), and both Children presenting Transversely—Abortion from Typhoid Fever in previous Pregnancy.* By Dr ANGUS MACDONALD, F.R.C.P.E., F.R.S.E., Lecturer on Midwifery and Diseases of Women and Children.

(*Read before the Obstetrical Society of Edinburgh, 25th February.*)

CERTAIN specialties of the following peculiar case have induced me to lay it before the Society in the form of a brief communication. The particulars of it are as follows:—

Mrs A., residing at 3 East M—— Place, Edinburgh, was married, at the age of thirty-one, on the 17th of July 1872. In September of the same year, whilst suffering from an ordinary attack of typhoid fever, she had a miscarriage at the middle of the second month of utero-gestation. She ceased to menstruate on the 14th of January

1873, and all went well, so far as she could judge, with her pregnancy, till the morning of the 22d of July last, when, on rising to pass water about 3 o'clock A.M., nearly a teacupful of blood escaped from her vagina. Alarmed at this occurrence, her husband came for me immediately, and I arrived at my patient's house at 4.30 A.M. I then found her in bed. Pulse regular and quiet, about 70 per minute, and the hæmorrhage entirely stopped. The patient had experienced no pain that morning. On examination, I discovered that her abdomen was much more distended than one would have naturally expected, considering the period of the pregnancy, and I was thereby led to suspect the possibility of the presence of either twin pregnancy or of hydramnios. Examination per vaginam was specially difficult, because the presenting part was uncommonly high, and the vagina was long and narrow. I was able to ascertain, however, that the cavity of the cervix was already obliterated, and the outer os distended to fully the size of a shilling piece. The anterior part of the lower segment of membranes presenting at the os, which part alone could at this time be reached, was *not covered* with any portion of placenta. There were occasionally slight uterine contractions, but so trifling as scarcely to attract the patient's attention. From inability to reach any portion of the placenta, or to feel any definable portion of the child, I was at first inclined to regard the case as most probably one of simple over-distention from hydramnios. The patient was, however, kept quietly in bed for a week, and an astringent mixture administered to her, as I considered it, on the whole, prudent to treat as for placenta prævia. About the same hour, on the morning of the 29th July, the bleeding recurred, while the patient was lying quietly in bed, and this time the hæmorrhage was much more severe. On arriving, in obedience to summons, I found the vagina full of clots, which I removed. I then ascertained that the cervix had become dilated since I examined last time to over the size of half-a-crown piece, but considerably less than that of a crown piece, but that its position was still very high up. Of the surface of the membranes thus laid bare by the dilatation of the cervix, about one-third—the left posterior third—was covered with a mass of placenta, whose free border was distinctly felt, and could be easily traced running from before backwards and inwards. A portion of the right side of the same membranous area was, at the same time, observed to be covered also with what seemed to be a mass of placenta. These masses of placentæ ran into each other posteriorly. Pains were now present, slight in force, but recurring at regular intervals. There was scarcely any hæmorrhage. These pains continued regular, but weak, and accompanied with occasional, but slight, bleeding throughout the day. About half-past 10 o'clock P.M., however, the contractions became much more severe, and, at the same time, the hæmorrhage somewhat increased. At 12.30, as the latter had now become rather severe, and the cervix was tolerably widely dilated, the presenting bag of membranes was ruptured, and the right foot of the first child, which presented transversely, was hooked down, and the breech brought to engage in the cervix. The bleeding then ceased almost entirely, whilst the pains became much more severe till the first child was expelled. It was a female, and living. The first placenta now prolapsed, and, becoming entirely separated from the second placenta, was expelled and removed. The bleeding from the remanent placenta being at this moment very serious and severe, the second bag of membranes was quickly ruptured, and a foot of the second child, which also presented transversely, pulled down, and delivery speedily effected. The second placenta quickly separated spontaneously, and there was no post-partum hæmorrhage. Though the patient almost fainted at the moment of completion of the birth of the first child, she rallied very well after the labour, and, with the exception of a somewhat anæmic expression for some weeks, and a rather quick pulse for a few days, her recovery was in every way normal, and she is now quite well. The first child lived for two days. The second made only a few imperfect efforts at breathing. Both, of course, were non-viable on account of prematurity.

As the subject of the bearing of typhoid and typhus fevers on pregnancy and childbed seems to me of great importance, and to have received an insufficient amount of attention from British obstetrical physicians, I mean, at the conclusion of this paper, to offer some observations on that point of this case, but will, in the first instance, take up the main questions involved in the paper.

In the first place, I would remark that the present case is one of very great rarity, and presenting a wonderful concurrence of complexities.

We had here two placentæ presenting both at the internal os, and belonging to twins, both of which, again, presented transversely. Then we had the onset of labour at a comparatively early period of the pregnancy, apparently due to the over-distention occasioned by the twins acting in conjunction with that preternatural tendency to prematurity of onset of labour so peculiar to placenta prævia.

I have never met with a case of twins previously in which the placenta presented, and can scarcely imagine it to be otherwise than exceedingly rare. Indeed, it does not need much mathematical knowledge to prove, from the known expectancy of the individual accidents, that the probability of such an occurrence happening is exceedingly small indeed. For, granting that placenta prævia is just as common in twin as in single conceptions, since twins occur not more frequently than 1 in 89 of all cases of labour, and placenta prævia not oftener than 1 in 500 of all cases of labour, the expectation of the concurrence of twins with placenta prævia would only be 1 in 500 × 89, or 1 in 44,500 cases of labour. But the expectation of the occurrence of twins with one placenta presenting at the internal os uteri must still be much greater—small though it be—than that of the accident with both placentæ presenting.

Again, taking into account the position of the two children, Kleinwächter[1] arrives at the result, that of all cases of twin births only in ·33 per cent. do both children present transversely, or about 1 in 26,969 of all cases of labour whatever. Then, if placenta prævia occurs in only 1 in 500 of all cases of labour, the concurrence of placenta prævia with the latter peculiarity must be 500 times less frequent, or 1 in 13,484,500 of all cases; and even this must be considerably more frequent than the concurrence of both placentæ presenting with both twins lying transversely.

Leaving this matter, however, which, it must be admitted, is more curious than useful, I may be allowed to observe that the concurrence of complexities in the case led to considerable practical difficulty, as it rendered the diagnosis of the case at first rather obscure and troublesome.

The transverse presentation of the children made them assume a distant position with reference to the cervix uteri, and allowed of the gravitation of fluid to fill the lower uterine segment. Thereby the os was very difficult to reach by the examining finger, and it was much more than usually hard to make out exactly how matters stood. Indeed, the whole abdomen was so largely distended in comparison to the advance of the labour, that although the probability of twins, or hydramnios, or either, with placenta prævia, was before my mind, and duly considered by me, I was, on the whole, inclined, after the first examination, to believe that it might turn out to be a case of simple hydramnios after all. The fact that I felt the anterior portion of the bag of membranes, and could reach that only, and found that it was entirely bare, added to the sources of error, and rather negatived the idea that I had placenta prævia to grapple with. I confess I did not pay much attention to the decision of the question so far as the point twins or no twins was concerned, and was never certain of twins being there till after the birth of the first child. Nor do I look upon such uncertainty as of any serious importance, because it can lead to no practical injury. The accoucheur can quite well afford to wait with patience till the question is solved.

But the question as between hydramnios and placenta prævia was both more important and beset with special difficulty. So far as regarded the mode in which the hæmorrhage occurred in even the first instance, everything pointed towards the greater probability of its being a case of placenta prævia. I accordingly treated the case as though it had been one of presentation of the after-birth. But, then, I had to put over against this fact, that the bleeding was not much, that it stopped at once, that I could feel a portion of the membranes through the partially dilated os the very first morning I saw my patient, and that that portion was entirely bare of placenta. The long narrow vagina, coupled with the high posi-

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[1] Lehre von den Zwillingen, S. 119.

tion of the cervix, prevented me from making a full and thorough exploration of the posterior circumference of the os, and of the lower posterior portion of the inferior segment of the uterus, else, I have no doubt, even then, I might have satisfied myself of the presence of placenta prævia. On the whole, however, I was rather inclined to believe that there was at least a chance that the bleeding might, after all, have proceeded from the rupture of some small cervical twigs through the opening up of the cervix from over-distention, probably due to hydramnios.

The second examination, with the cervix somewhat more dilated, however, revealing the well-known spongy masses of placenta on the posterior and lateral aspects of the presenting membranes, removed of course all doubt, and justified the treatment which had been adopted as the safer alternative.

As to the subsequent treatment of the case, I have really very little to say. I proceeded according to the ordinary principles of treatment in such circumstances, which are to effect delivery as soon as possible, and as safely as possible. In pursuance of these objects, I had to hurry the labour at first somewhat, and subsequently to finish it very quickly.

So long as I could wait without any risk to the life of the mother, I delayed, so as thereby to gain room, by the natural efforts of dilatation, for the transmission of the child. The bleeding never actually became urgent till the dilatation of the lower extremity of the uterine ovoid had so far advanced as to allow of the breech being at once drawn down and brought to engage in the cervix. I thereupon ruptured the membranes, and, finding a side presenting, hooked down a leg, turned easily, and pulled the breech down. The bleeding was then practically arrested till the first child was born by the mechanical pressure of the body of the child. Then, however, the hæmorrhage from the uterine surface, thus divested of this natural tampon, and laid bare by the prolapse of the first placenta, and partial separation of the second, became positively alarming, and would have undoubtedly proved suddenly fatal, had not the labour been at once terminated by the artificial delivery of the second child, the removal of the second placenta, and subsequent contraction of the uterus.

A case such as the present, had it simply been one of single presentation of the placenta, might have been expected to be a very easy one.

I. Because the presentation of the placenta was partial, and would not consequently necessitate a very large spontaneously-detaching area.

II. Because the children were both small and premature, and on that ground also might have been fairly expected to pass through a comparatively small aperture, which consequently would lead to a still smaller amount of spontaneously-detaching, *i.e.*, of bleeding area being needed.

Two peculiarities, however, removed the present case out of its normal category.

I. The fact that the presentations were transverse, preventing the descent of the head or breech, would have led to the continuation of dilatation and detachment long after the period when either head or breech would have passed the os uteri.

II. Though both placentæ reached the *edge* of the internal os *only*, yet the two masses occupied a much larger proportion of the spontaneously-detaching area than would have certainly been the case with a single placenta, with its apex at the inner os and its base on the sides of the body of the uterus; so that though the case, anatomically considered, is one of partial presentation of the placenta, yet practically it approximated in danger to the complete accident.

In speaking of the spontaneously-detaching area, as well as in discussing this subject in general, I cannot refrain from expressing my deep sense of the obligation which the obstetrical profession owes to the President of this Society for his recent contributions to our knowledge of the laws regulating this accident.

As, no doubt, most of the members of this Society are aware, in a paper read in October last year before the Obstetrical Society of London, and published in the last Heft of the *Archiv für Gynäkologie*, Dr Duncan has advanced an entirely new view regarding the natural mechanism of the separation of the placenta from the uterine surface in cases of placenta prævia. In that contribution Dr Duncan maintains, and supports by arguments, which to my mind at least seem incontestable, that the spontaneous detachment of the placenta when it presents is not due to contraction or diminution in all directions of the placental area, but to stretching of its uterine attachment through the opening of the lower segment of the uterine ovoid, to form a passage for the child.

In following out this view, Dr Duncan has defined, more rigidly than Dr Barnes and others who have travelled this ground before him, the exact limits of the spontaneous detaching area. According to him, the placenta, in an ordinary case of complete placenta prævia, with a full-grown child at full term, ceases to suffer any further separation when the dilatation of the lower segment of the uterus attains a diameter of 11 centimetres, *i.e.*, nearly 4½ inches. This amount of dilatation, as the result of careful measurements made by Dr Duncan, is shown to coincide with the circumference of a circle whose plane is at right angles to the longitudinal diameter of the uterus, and whose diameter measures 11 centimetres, whilst every part of its circumference, measured along a meridian line of the uterus, is 6 centimetres distant from the central point of the internal os uteri.

It follows, therefore, that in a case of placenta prævia such as I have been describing, if the head had presented, hæmorrhage, due to separation of the placenta, would certainly have ceased some time *before* a segment of the spheroid, formed by the lower extremity of the uterus, with a chord of 11 centimetres, and an arc of 12 centimetres, had been obliterated by the natural dilatation.

I say *before* advisedly, because, even though the case had gone on to full time, such a segment would be the maximum amount of dilatation necessary; whereas in the labour which we have been considering, the smallness of the children rendered the amount of dilatation necessary considerably under a segment with a chord of 11 centimetres.

We are thus, through the labours of Dr Duncan, brought to such a degree of certainty, or at the very least of approximate certainty, in respect to the natural limit of bleeding in cases of placenta prævia, which is a very great advance upon the bewilderment induced by theoretical ideas regarding indefinite and indefinable zones.

But notwithstanding all that has been previously written upon this subject, and making full allowance for the above-mentioned and the three other able and more recent contributions of our President to the *Edinburgh Medical Journal* and the *British Medical Journal*, in regard to the origin and mode of arrestment of this kind of hæmorrhage, much has still to be done, as the whole subject is still enveloped in very much mist.

If I might venture a criticism upon Dr Duncan's theory of the origin and mode of arrestment of bleeding in such cases, which has been lately put forward and ably advocated by him, it is that I think he has paid too little attention to the curling arteries as a source of such bleeding, and too much to the venous sinuses in the placental area.

Dr T. Snow Beck, in the *Obstetrical Journal* for December 1873 and January 1874, has endeavoured to revive the views originally brought forward by Dr F. W. Mackenzie on this subject. He maintains, supporting his arguments especially by reference to Dr Mackenzie's experiments, that in bleeding from the uterine surface the bleeding is arterial and direct. On the other hand, Dr Duncan would have it an indirect oozing, the arteries inside of the uterine wall bleeding first into the sinuses, and then the sinuses discharging their blood into the cavity of the uterus through its free surface. Dr Duncan does not hold that the bleeding is regurgitant, in the sense that it is a reflux from the veins leading from the uterus towards the heart back again into the uterine sinuses.

Now, though I have not sufficiently studied this exceedingly intricate subject so as to be able to take up the strongly-affirmative position defended by Dr T. Snow Beck, I must confess that my leanings are towards his views; and I consider the facts, experiments, and arguments advanced by him, deserve a larger amount of attention from the thoughtful accoucheur than they have as yet received. It would be a great gain if the profession thereby were only led to abandon the exceedingly unphysiological and provedly hazardous treatment of post-partum hæmorrhage by means of injections of perchloride of iron, and to get free of belief in the dangerous nature of the exceedingly effective—and, as I am glad from ex- ded experience for several years back to be able to testify, in perfect accordance with the views of Dr T. Snow Beck—completely innocuous mode of arrestment of hæmorrhage post-partum by injection of cold water into the uterus.

But this is rather departing from my subject. In reference to the course of the bleeding, I would beg to state that the late Professor Goodsir—than whom, I am sure the members of this Society will allow, a more patient, more able, or more careful observer never lived—used in his lectures on the anatomy of the uterus to substantiate the views of Owen and of Simpson in regard to the platform arrangement of the uterine veins, and to describe the manner in which the successive platforms opened into each other obliquely, with the free edges projecting in such a manner as to act substantially as valves, and thus to prevent the escape of blood from a higher platform to a lower. I feel convinced that though it is difficult to understand how such small vessels, numerous though they be, as the uterine curling placental arteries, may give rise to such severe bleeding, both in connexion with placenta prævia and in hæmorrhage post-partum, yet the difficulties, on the other hand, to account for the arrest of hæmorrhage from the open uterine sinuses without the contraction of the organ, are still more difficult to meet.

In regard to this point, I do think that the existence of some such anatomical arrangement of the veins, such as Owen and Goodsir pointed out, gets us more rationally and easily over the difficulty than to trace it to the obstruction due to a change of shape in the uterine sinuses effected in the course of the dilatation of the uterine placental area, consisting in the elongation of them in the lateral direction, and the approximation of their walls in the longitudinal direction, as Dr Duncan urges. I do not think that Dr Duncan, even when backed up by the great and justly-respected name of Professor Tait, proves that point well. This part of Dr Duncan's paper is to me not at all convincing.

Before such change in the shape of the discharging orifice could be effective as a hæmostatic means, I feel certain that we would need a diminution of the discharging area in all directions; for I cannot imagine the mere friction of the opposed surfaces of openings, so large as those sinus openings are, to be able to resist the degree of tension, often great, to which the maternal blood in the uterine walls is subjected, both in placenta prævia and otherwise. In the case of the small curling arteries again, supposing them to be sources of hæmorrhage, we have the greater contractility of their walls, the fact, also, that the separation is by tear and not by cutting, and the exceedingly small diameter of the vessels, uniting, to afford conditions specially favourable for the process of nature's hæmostatics. Only in cases where the general blood-pressure was specially exalted, or in which the retentive power of the abdomen (so ably and convincingly urged by Dr Duncan as one of the means by which the bleeding is arrested from the free surface of the uterus, in cases of placenta prævia) was defective, would the natural hæmostatic means afforded by the torn surface of those arteries be expected to fail, provided we could account for occlusion of the sinuses by any certain means. That the veins inside the uterus have such an arrangement as could make their various platforms incapable of transmitting blood from a higher to a lower level is my contention, supported by the experiments of F. W. Mackenzie, and the dissections of Owen and Goodsir.

To make certain of the ground maintained by Mackenzie, T. Snow Beck and myself, however, we need extended observation and fresh dissections; and, while criticising this view of Dr Duncan, I feel constrained to assert, that it is much easier to find fault with his, than to suggest a better explanation for the arrest of the hæmorrhage. Will no one attempt to tell us why placenta prævia so very frequently gives rise to miscarriage?

Let me now draw the attention of the Society to another point of great practical value, involved in this patient's history, viz., that her previous pregnancy was interrupted by typhoid fever. The undoubted importance of this subject, as well as the great amount of uncertainty in the professional opinions regarding it, must form my apology to the Society for directing their attention to it for a few minutes.

As the patient was not then under my care, it would not be in good taste (and, besides, I have not sufficient knowledge of the course the disease ran) for me to go into the details of that occasion. Suffice it to say, that the patient aborted in the course of an attack of typhoid fever, and that, notwithstanding, she made an excellent recovery.

In so far as concerns the bearing of typhus and typhoid fevers upon the pregnant and puerperal female, I am surprised to find in the usual authoritative sources the most contradictory statements.

From what I have been able to gather, however, as well as from my own experience, I am led to affirm, in as emphatic a manner as possible, that while neither the pregnant nor the puerperal patient seem to be specially liable to these diseases, yet the researches of Hecker, Scanzoni, Wallichs, Kaminsky, and others prove that pregnancy forms no protection against imbibing the poisons of typhoid or of typhus fever. It must be allowed, however, that the divergence of opinion of professional authorities on this subject is very great, and really very unintelligible. It is curious, also, to notice the gradual advance of medical opinion on this matter.

Not many years ago, it was believed that pregnancy afforded almost absolute immunity against typhoid fever. Thus, Rokitansky[1] wrote, in the year 1842, "that pregnancy conditions an almost absolute immunity against the typhus (typhoid) process." In 1865, Niemeyer[2] says, in his article on typhoid fever, "Pregnant women and women giving suck are seldom seized; the puerperal state secures an almost complete immunity against abdominal typhus." In this opinion, it is right to state, however, that Niemeyer differs *in toto cœlo* from the great majority of his countrymen, as will afterwards be shown, by referring to Hecker's paper. Among British authors there seems, so far as I can find out, a very great paucity of remark on the subjects, both of abdominal and exanthematic typhus, in reference to pregnancy and childbed. It would appear that no doubt had ever existed in regard to the probability of such females taking the fever, but that little attention had been given to the subject, except in reference to the probability of abortion and the probable prognosis. Dr John Harley, in his article on Enteric Fever, in Reynolds's System of Medicine, vol. i. page 358, states that "abortion is almost certain to occur if a pregnant woman be attacked with enteric fever. The only two pregnant women who have come under my care aborted the one at the third month of gestation, the other at the fifth. Both recovered well. Phlegmasia dolens is apt to be a secondary complication in such cases." In his article upon Typhus Fever, *opere citato*, p. 440, Dr George Buchanan states, "that when pregnant women get typhus, if they are past the sixth month, they frequently miscarry; but this accident adds very little to the danger of the case, typhus fever therein differing much from typhoid or scarlatina." On the other hand, Scanzoni[1] mentions ten cases of typhoid fever, treated by Fingre Ritter and himself, six of whom aborted, and yet the abortion was not followed by any unfavourable turn in the course of the disease. Also, Wallichs[2] records two well-marked cases (the one an excessively severe and complicated one) of typhoid fever in women far advanced in pregnancy. Of these, the first carried her child to the full term, the latter aborted about the beginning of the eighth month. Both patients, however, did well.

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[1] Path. Anatomie, III. Band, S. 258.

[2] Lehrbuch der Speciellen Pathologie und Therapie, 6 Aufl. 1865, Band [illegible], S. 578.

Kaminsky of Moscow published in the Russian language a work, entitled, "Contributions to the Knowledge of the Influence of Typhus and Remittent Fevers upon the Course of Pregnancy," in which he asserts that he had seen 87 cases of pregnancy complicated with typhus or remittent fever, within a year. Of these, 55 were during the first half, and 32 during the second half of pregnancy. A reference is made to a German translation of this paper, but I have been unable to obtain the work referred to. From an abstract which Wallichs, *in loco citato*, gives of the paper in question, it is impossible to make out, however, what proportion of those cases were exanthematic typhus, abdominal typhus, or remittent fever.

It is evident, however, that in Russia, at least, it is no rare thing to find typhus or typhoid combined with pregnancy. In an exceedingly interesting paper, published in 1866, Hecker[3] maintains that abdominal typhus is very common in the Lying-in Institution of Munich, and maintains that many of these cases of fever and diarrhœa during the childbed period, which, though they present no inflammatory lesion whatever, in either the uterus, its adnexa, or the abdomen, are usually regarded as mild puerperal fever, are nothing more nor less than abdominal typhus. This view he maintains with much ingenuity, and supports it further by reference to five post-mortem examinations of patients who had died of typhoid fever shortly after confinement. He maintains, in direct opposition to Niemeyer, that the lying-in period really predisposes to the reception of the poison of typhoid fever, though his *sectiones* do not help him much in the proof of that assertion, inasmuch as, according to the just criticism of Wallichs, every one of these five cases, on Hecker's own data, must have been confined whilst the fever-poison was in the system.

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[1] Lehrbuch der Geburtshülfe, IV. Auflage, II. Band, S. 13.

[2] Monatsschrift für Geburtskunde, XXX. Band, S. 253.

[3] Monatsschrift für Geburtskunde, XXV., II. Band, S. 423.

As corresponding with this view, but in regard to ordinary typhus, we may here quote the younger Ramsbotham,[1] who writes thus: "Another disease which has been confounded, in consequence of the name applied to it, with peritonitis, is a fever allied to typhus, of the most severe and aggravated form. To this the term puerperal fever would be peculiarly proper, and it might be retained, if it had not already been employed in so vague and undefined a sense. Typhus, indeed, is very rare in the puerperal state, the most uncommon, perhaps, of all the affections which have been described under this denomination. It commences at the time after delivery most usual for serious diseases to begin—about the second, third, or fourth day. It is, perhaps, ushered in by shivering, but this is by no means always the case. This is followed by pain in the head and along the spine, accompanied with great depression of spirits, mental agitation, and inquietude," etc., etc.

He afterwards relates the particulars of the outbreak of what he —without, it must be confessed, any very convincing evidence— regards as an outbreak of puerperal typhus.

In the summer of 1872, I wrote my friend Dr John A. M'Dougall of Galashiels to ascertain his views on the bearing of zymotic diseases upon pregnant and puerperal females; and, in a brief note in reply, he stated that he had had several cases of typhoid fever combined with pregnancy and the puerperal state, and, with one exception, they recovered. He had had two cases of typhus, and both had died. He was inclined to regard typhus as equally fatal with scarlet fever.

My own experience of either fever, in relation to pregnancy or the puerperal condition, has been small. Two and a half years ago I attended a lady who had a mild attack of typhoid fever during the fifth month of utero-gestation. Her pregnancy went on to full term, and she gave birth to a healthy boy.

In August last year, I was present in consultation at the birth of child, at about the middle of the eighth month, which took place on the nineteenth or twentieth day of a well-marked case of typhoid fever. Both mother (who was a primipara) and her child did well.

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[1] On Obstetric Medicine and Surgery, p. 622.

Some years ago, I attended, in conjunction with one of my dispensary pupils, a patient in 18 Greenside Row, who, after an easy labour, fevered on the third or fourth day, and whose case turned out to be one of well-marked exanthematic typhus. She recovered well; and there were in her case none of those alarming symptoms referred to by Ramsbotham.

In January 1871, I attended a patient who was seized with a severe attack of typhus while nursing. She sank on the thirteenth day of the fever from exhaustion, but the course of this disease was that of an ordinarily severe attack. This case is mentioned to show that exanthematic typhus occurs in women who are giving suck.

I recollect, when I was a student, in the Edinburgh Infirmary there occurred a case of typhus in which the patient aborted and died immediately after.

Indeed, from all I am able to gather from these records, I am led to believe that neither pregnancy nor the puerperal condition give any immunity against the onset of typhus or typhoid fever. Pregnant women, women in the childbed week, or women giving suck, seem to be seized with these fevers in proportion to exposure to the virus just as readily as other people.

In case a pregnant woman is seized with either of these fevers, it is undoubtedly proved that she is exceedingly liable to abort. In case of her aborting, it would appear that there are no just grounds for the opinion, that typhoid is very dangerous, and typhus not, when we look at the statistics of Hecker, Scanzoni, and others.

In case of the coexistence of pregnancy with either typhoid or typhus fever, the patients are unquestionably in considerably greater danger than if pregnancy were absent; but typhus would appear to be just about the same in risk as typhoid, neither being necessarily fatal.

In connexion with the lying-in period, both these fevers appear to be much less fatal than measles and scarlet fever.

ARTICLE V.—*The Use of the Electro-Magnetic Current in Labour: Notes of Two Cases.* By A. E. M'RAE, C.M., M.D., Penicuik.

(*Read before the Obstetrical Society of Edinburgh, 11th March.*)

ON a previous occasion,[1] I called your attention to the electro-magnetic current as an additional aid in the second stage of labour. I pointed out that by the use of the interrupted current applied to the abdomen and the perineum, uterine pains could be induced and intensified; that it was of use when the uterus was exhausted, or refused to act by other means of stimulation; that no danger was to be feared by its use; and that its application was pleasant to the patient. I also pointed out when it should be used, and when its action was uncalled for. Having also detailed the proper method of application, I will not now occupy your time by recapitulating, but bring before your further notice the notes of two cases that have occurred in my practice during the past year, wherein I used the electro-magnetic current to expedite the second stage of labour.

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[1] See Journal for September 1873, page 243.

CASE IV.—Mrs A., multipara, began to feel parturient pains on the evening of 20th August 1872. She was at full time. I saw her about 1.30 A.M. on the morning following; on examination, I found the os about the size of a shilling. The head could be felt very far up. During the morning the pains increased in severity and force, the os slowly dilated, but the head did not descend. By ten o'clock, however, the head came into the brim; but though the pains continued strong, it did not engage in it, but retreated immediately on the cessation of the pain. At ten o'clock A.M., she insisted upon having chloroform, having always been delivered under it before. Under the chloroform, I got the os completely dilated, and used Ziegler's forceps, but could not get them to engage the head, which, though pressure was kept up externally by the attendant, still continued to retreat on the cessation of pains. At the end of an hour the pains ceased. I now left off for some time. At twelve I applied the electro-magnetic current, placed the negative pole on the abdomen, and the positive on the perineum. A strong pain was induced, but was scarcely felt by the patient. The uterus, however, contracted, and the head again came down on the brim. I continued the current for about one and a half hours, and intermitted now and again so as to simulate the natural pains. When the current was interrupted, no pains occurred; when it was applied, pains came. The longer the current was used, the stronger became the pains, and the more strongly did the uterus contract. By keeping up pressure externally when the current was intermitted, the head was got to engage in the brim; but for some reason then unknown, but suspected, the head would not come down. After a prolonged trial of the current, both with and without chloroform, I deemed it advisable to adopt some other method. Accordingly, I again applied the forceps. The head had to be pushed back to admit them, but I could not get a hold sufficiently firm to warrant their use. As there was no danger to fear at this stage, the head not pressing unduly upon any important part, I thought it better to leave off, at least for a time. And after she had somewhat recovered from the effects of the chloroform, I administered twenty drops of chlorodyne. This, however, was succeeded not by sleep, but by severe uterine pains; the head engaged in the brim, passed into the hollow of the sacrum, and the labour was completed by the forceps in fifteen minutes after the chlorodyne had been administered. The cause of the delay was now ascertained. The child t full period and of average size. The cord measured 12½ inches nbilicus to placenta, and was also engaged round the child's

neck. This condition of things had given rise to the great tendency to retraction of the head, had also prevented the head engaging in the brim, and until the placenta had become detached, or the uterus had become somewhat inverted, had been the chief factor in delaying the labour. It was also the cause of considerable difficulty in getting the shoulders into the world. I found the placenta detached, and removed by slight traction. It presented no features of irregularity.

I do not wish to digress from the idea of this paper, and remark upon the short cord, and its relation to the neck, as a cause of delay in the labour, but I bring the case forward to prove a proposition that I communicated to this Society some months ago, that the electro-magnetic current induces, invigorates, and intensifies uterine pains,—a doctrine that has been denied in obstetric science by very high authority, and by a late member of this Society. The facts as related above are true, and go to prove the utility of the electro-magnetic current in cases where the uterine action is deficient. And although the labour was not accomplished by its use, the action of the uterus during its application completely demonstrated its power.

Case V.—Mrs M'L., aged twenty-two, a primipara, and under the average size, had been in labour for thirty-two hours when her medical attendant requested me to see her at 10 A.M. on the 7th of October 1872. The pains had ceased for three or four hours, and various agents had been used to restore them, but they were useless. I found the head upon the brim, but not entered. I applied the current at the request of her medical attendant. In a few seconds the pains returned and uterine action commenced, and little by little the head came down. The current was used now and again for three-quarters of an hour, when the head got impacted in the brim; and although the uterus continued to contract vigorously, the head became immovable. The current was now abandoned. The patient was put under chloroform, and with great difficulty the forceps were adjusted on the head; but so disproportionately large was the head to the passages, that an hour elapsed before the head was born. The force required to extract it was very great indeed, and was accomplished only by the exhibition of great muscular exertion. My colleague, a powerful man, was unable to extract it. Without the exhibition of this force, craniotomy would have had to be performed. The child was a large one; and, notwithstanding the force used, no damage was done to the head or to the passages, and the patient made a good recovery.

Although, in this case also, the labour was not accomplished by the application of the current, yet a sufficient manifestation was given of its utility in cases of inertia uteri.

Neither of these two patients complained of pain, but rather seemed to like the feeling. It appeared also that the uterus contracted violently, without causing so much pain as when it contracts without the current. Its use need not be feared, therefore, by the most tender of mothers.

ARTICLE VI.—*Valedictory Address to the Medico-Chirurgical Society of Edinburgh, delivered on the occasion of retiring from the Chair of the Society, 7th January* 1874; by P. D. HANDYSIDE, M.D., F.R.S.E., formerly Senior President of the Royal Medical Society, and Senior Acting Surgeon to the Royal Infirmary; Teacher of Anatomy in the Edinburgh School of Medicine. Part III.

(*Continued from page* 912.)

IX. SURGERY

(including *Ophthalmic*, *Aural*, and *Dental Surgery*).

(1.) *Surgery Proper.*

AMONG our earliest annals in SURGERY are the following monographs:—In 1821, *Mr James Syme* read a "Case of Diffuse Inflammation of the Cellular Texture" (Trans., i. 580). In 1822, *Dr Adam Hunter* presented a paper, "Some Remarks on Dislocations of the Hip and Shoulder Joints, founded upon the Dissections of two recent Cases of Unreduced Luxation;" and *Dr Richard Huie* read one "On a Polypus of great size Removed by him from the Root of the Tongue by Ligature." In 1823, *Mr John M'Farlane* read a "Case of Cynanche Parotidea, fatal by Hæmorrhage." In 1824, *Dr Barlow of Bath* read, "On Severe and Long-protracted *Stricture*, in which relief was obtained by an Incision into the Membranous Portion of the Urethra;" and *Mr P. C. Blackett*, Surgeon Extraordinary to the Duke of Clarence, read a "Description of a Bullet Extractor, which he presented to the Society." In 1825, *Mr George Fielding of Hull*, "On a new substance, Silkworm Gut, for securing Divided Arteries;" and *Dr Boggie*, Surgeon to the Forces, "Observations on Hospital Gangrene." In 1826, *Dr George Ballingall*, "Case of the High Operation of Lithotomy;" *Mr J. Brown*, Surgeon to the Forces, "Case of old Stricture, extensive Sloughing of Urethra through an opening above Pubes;" *Mr W. A. Dalziel, R.N., Wigtown*, "Case of Fracture of the Cranium, followed by Hernia Cerebri;" and *Dr Alex. Ramsay, Dundee*, "Case of very large Calculus Extracted from the Female Bladder by Dilatation." In 1827, *Dr Huie*, "Case of Polypus of extraordinary size Removed from the Root of the Tongue by Ligature, with Remarks—and the Preparation;" *Mr Copeland Hutchison*, "On Hernia Cerebri;" *Dr Wm. Cullen* gave a "Case of Laryngitis, in which Bronchotomy was successful;" and *Dr James Simson*, "Case of Strangulated Umbilical Hernia in the Adult in which the Operation was successful." In 1828, *Dr Brulatour of Bordeaux*, "Observations on a Case of Fracture of Cervix Femoris, cured, with subsequent Dissection." In 1829, *Dr W. Cumin of Glasgow* gave "Remarks on Stricture, with three Cases of Stricture of the Œsophagus." In 1830, *Dr Francesco*

Bennata of Padua read, "On some of the Diseases of the Throat affecting the Organ of the Voice, especially in persons obliged to Speak or Sing in Public." In 1831, *Dr Abercrombie* gave a "Case of Periostitis of the Cranium successfully treated;" *Dr Russell,* "On Subluxation of the Orbicular Joints, with a Case;" and *Sir George Ballingall,* "Two Cases of apparently Spontaneous Gangrene in the Lower Extremities, which occurred in Edinburgh Castle last winter."

The following include nearly all the other Surgical communications:—In 1832, *Dr John Campbell,* "Case of the Destruction of the Eyelids, the singular appearance of the Eyeball shown in a Drawing;" *also,* "Case of Contraction of the Mouth remedied by an Operation;" *also,* "Case of Successful Amputation in a Case of Spreading Gangrene of the Lower Extremity;" *also,* "Artificial Anus after Inguinal Hernia." In 1833, *Sir George Ballingall,* "Case of Rupture of the Tendon of the Biceps of the Arm." In 1834, *Dr John Taylor,* "Case of Prolapsus of the Gut of unusual magnitude;" and *Mr Craig, Ratho,* "Two Cases of Inflammation of the Parts lining the Orbit." In 1835, *Dr John Gairdner,* "Case of Severe Injury of the Abdomen, terminating fatally, with Dissection." In 1836, *Dr Knox,* "Case of Painful Crepitation in the course of the Radial Extensor Tendons;" and *Dr William Macdonald,* "Notice of a new Saw for Amputation." In 1838, *Dr P. D. Handyside* read a "Case of Anchylosis of the Occiput, Atlas, and Vertebra Dentata;" *Mr Watson,* "Case of Popliteal Aneurism in a Female;" and *Dr King of Barbadoes* communicated a "Case of Recovery after the Arm, Scapula, and portion of the Clavicle had been torn off by Machinery, with Remarks by *Dr P. D. Handyside.*" In 1839, *Mr George Glover,* "Case of a large Brass Ring found on Dissection over the Rima Glottidis, where it had remained embedded for two years;" *Dr Maclagan,* "Case of Epilepsy and Deafness, apparently depending on the Presence of a Foreign Body in the Ear;" and *Dr John Gairdner,* "Case in which the Operation for Strangulated Hernia was performed on a New-born Infant affected with Congenital Exomphalos." In 1841, *Dr Alex. Watson,* "Remarks on Tracheotomy, with Cases;" *also,* "Diaphragmatic Hernia from Fractured Rib;" and *Mr Benj. Bell,* "Case of Double Psoas Abscess." In 1842, *Dr Allan, Haslar Hospital,* read "Cases of Hæmorrhagic Diathesis." In 1843, *Mr Spence,* "Case of Ligature of Common Carotid, with some General Remarks on the Ligature of Arteries;" *Dr Alex. Watson,* "Cases of Fracture of the Lower Jaw;" *Dr P. D. Handyside,* "Case of successful Amputation at the Hip-Joint, with Illustrative Preparation and Casts;" *Dr Monro, Dundee,* "Remarkable Case of Vesical Calculus;" and *Dr Jackson of Leith,* "Case of Death from a Piece of Potato-skin in the Larynx." In 1844, *Mr Syme,* "On the Treatment of Stricture of the Urethra in cases resisting other Means of Remedy by External Incisions, Free and Subcutaneous;" *also,* "On a New Mode of treating Fungus of the Testicle." In 1845, *Dr James Allan of Haslar Hospital,* "Case of Aneurism successfully treated by Pressure;" *Dr A. King of Glasgow,* "Case of Avulsion of the Left Arm and Scapula, with Recovery of the Patient;" *Mr Syme,* "Notice regarding Amputation of the Knee-Joint;" and *Dr Parker of Canton,* "Notes of Surgical Practice amongst the Chinese." In 1847, *Mr Syme,* "On the question of Ligature and Compression in the Treatment of Popliteal Aneurism;" *also,* "On Restoration of the Upper and Under Lip;" *Mr C. H. Hallet,* "Subluxation of the Humerus, Forwards and Inwards;" *Dr P. D. Handyside,* "Case of a Fatal Result *ten hours* after Fracture of the Bodies, and Dislocation of the Arches, of the Fourth and Fifth Vertebræ;" and *Mr Spence,* "Case of Ligature of Axillary Artery for Hæmorrhage from Burn of the Arm." In 1848, *Dr James Duncan,* "Cases of Hernia, with Remarks;" *Mr Miller,* "On the Treatment of Ununited Fracture by Subcutaneous Puncture;" and *Mr Spence,* "Case of Oblique Fracture of the Femur above the Condyles, with a Cast after Dissection." In 1849, *Dr Richard Mackenzie,* "On Amputation at the Ankle;" *Dr Taylor,* "Case of Fracture of the Pelvis, with Injury of the Bladder;" *Dr Andrew,* "Case of Fractured Pelvis, complicated with Pregnancy;" *Mr Syme,* "Case of Internal Hernia remedied by Operation;" *also,* "Amputation of the Upper Part of a Necrosed Femur at the Hip-Joint;" and *Dr P. D. Handyside,* "Fatal Injury of the Abdominal Wall and Rectum, etc., by the Pole of an Omnibus." In 1850, *Mr William Brown,* "Letter on the Treatment of Stricture by the late John Walker, F.R.C.S.;" *Dr Gillespie,* "Case of Laryngitis, with Expulsion of False Membrane of the Larynx;" and *Dr John Scott,* "On the Treatment of Affections of the Larynx." In 1851, *Dr Mackenzie, Mr Miller,* and *Mr Syme,* communicated Cases, favourable and unfavourable, of the "Perineal Section;" and *Dr Mackenzie,* "On the Restoration of the Upper Lip." In 1853, *Dr Mackenzie,* "On Excision of the Knee-Joint;" and *Mr Syme,* "On the Pedunculated Exostosis of the Long Bones." In 1854, *Mr Syme,* "On Velu-Synthesis, or the Operation for Cleft Palate;" *Dr John Traill, Arbroath,* "Case of Amputation of the Foot without removal of the Astragalus;" *Mr Miller,* "Case of Aneurism unsuccessfully treated by Compression;" *Mr Sidey,* "Case of Spontaneous Gangrene in a Child eight months old;" and *Dr Gillespie,* "Case of Cancer successfully treated by External Application of Caustic." In 1855, *Dr James Johnston, Hampshire Militia,* "Some of the more interesting Cases among the Wounded from Alma;" *Mr Spence,* "Case of Hernia, complicated by Irregularity of Obturator Artery;" and *Mr Miller,* "Unsuccessful Treatment of Loose Cartilage in the Knee-Joint." In 1856, *Dr Milroy, 30th Regiment,* "Some Cases in Military Surgery occurring in the Crimea;" *Dr Gillespie,* "On Condyloma, its Pathology and Treatment;" and *Mr David Yellowlees,* "Case of Ruptured

Bladder, with Remarks by Dr W. T. Gairdner." In 1858, *Mr Spence*, "Cases illustrative of Gunshot Injuries (or Wounds) occurring in Civil Practice;" and *Mr Lister*, "On Gangrene from Arteritis." In 1859, *Mr Jardine Murray*, "Case of Extraction from the Pharynx of a Needle which had penetrated the Neck;" *Dr P. H. Watson*, "Two Cases of Tracheotomy on account of Laryngeal Obstruction;" *Mr Spence*, "Cases of Croup in which Tracheotomy was performed—a Supplementary Paper;" and *Dr James Young*, "Case of Hydrocele cured by the Wire Seton." In 1860, *Dr Scot, 79th Highlanders*, "Case of Ligature of External Iliac for Hæmorrhage from Sloughing Phagedæna;" *Dr James Struthers of Leith*, "Case of Amputation of the Thigh in which Acupressure was successfully employed;" *also*, "Case of Excision of the Mamma, with a similar Treatment and Result;" and *Dr P. D. Handyside*, "Case of Amputation beneath the Trochanters for Spreading Gangrene, in which Acupressure was successfully employed." In 1861, *Mr Syme* exhibited a "Patient in whom he had successfully performed the Operation of Invagination of a Cellulo-Cutaneous Flap for the Cure of Oblique Inguinal Hernia, by a method less complicated than that of M. Wützer;" *Dr Gillespie*, "The Wire Seton in Hydrocele;" and *Dr Myrtle*, "On the Treatment of Burns and Scalds." In 1862, *Dr P. D. Handyside*, "On Acupressure, illustrated by a Second Case of Amputation beneath the Trochanters, in which a successful result followed Occlusion of the Arteries by Acupressure alone;" *Mr Spence*, "Notes of a Case of Fracture of the Neck of the Scapula, simulating Dislocation at the Shoulder-Joint—Preparation shown;" *Dr Watson*, "On the Radical Cure of Exomphalos in the Adult;" *Mr Spence*, "Case of successful Amputation at the Hip-Joint for Medullary Disease of the Thigh, with a preparation of the Tumour;" *Dr Gillespie*, "Case of Depressed Fracture of the Sternum, with Abscess of the Anterior Mediastinum, and Compound Comminuted Fracture of the Lower Jaw, terminating successfully after an attack of Pyæmia;" and *Dr Keiller*, "On Cancrum Oris." In 1863, *Mr Alexander Neil, 65th Regiment*, "On Gunshot Wounds, with Remarks on their Treatment;" *Mr Spence*, "On Amputation;" *Dr Thomas Keith*, "On Ovariotomy, with Cases;" and *Dr Archibald Hamilton, Carlisle*, "Report of some Cases of Amputation in which Acupressure was employed." In 1864, *Mr Peter Brotherston, Alloa*, "Case of Removal of a Portion of the Humerus;" and "Case of Gunshot Wound of the Right Side, with Fracture of the eighth and ninth Ribs, and Laceration of the Liver;" *Dr W. B. M'Kinlay of Paisley*, "Case of Ligature of the Common Iliac Artery for Secondary Hæmorrhage after Amputation;" *Dr P. H. Watson*, "Two Cases of Injury of the Urethra, with Complications;" *Mr Spence*, "Successful Case of Primary Amputation at the Hip-Joint—Patient shown;" *Mr Annandale*, "On Cleft Palate;" and *Dr Turner of Keith*, "Tracheotomy twice performed for Strumous Laryngitis; Pertussis while wearing the Canula—Recovery." In 1865, *Dr Alex. R. Simpson,* "Death from Rupture of a Varicose Vein;" *Dr J. Crichton Browne,* "Case of Foreign Body in the Œsophagus of an Idiot, attended by some Anomalous Symptoms;" *Dr P. D. Handyside* gave a "Successful Case of Transfusion of Blood in a Case of Excessive Hæmorrhage after Compound Comminuted Fracture of Tibia and Fibula;" and *Dr W. B. M'Kinlay,* "Acupressure, illustrated with Cases." In 1866, *Dr John Duncan* "On the Galvano-puncture of Aneurisms;" *Mr P. K. Vartan, Medical Missionary at Nazareth,* "Case of successful Amputation for Elephantiasis, in which Acupressure was employed;" *Dr Watson,* "On the After-treatment of Excision of the Knee-Joint;" *also,* "Apparatus to Obviate Deformity of Fracture of Lower Third of Radius;" *also,* "Second Communication on Excision of the Knee-Joint;" and *Dr John Duncan,* "Case of Ununited Fracture of the Ulna." In 1867, *Dr Joseph Bell,* "Case of Pulsating Tumour in the Orbit, cured by Ligature of the Common Carotid Artery;" *Dr John Duncan,* "On the Treatment of Aneurism by Electrolysis;" and *Mr Annandale,* "On the Treatment of Club-foot." In 1868, *Dr Joseph Bell,* "A Case of Necrosis of the Femur, involving the whole thickness of the Shaft, with Disease of the Knee-Joint—Recovery, with a useful Limb;" *Mr Annandale,* "Case of Ruptured Perinæum with Prolapsus Vaginæ, cured by operation;" *Dr Watson,* "On the Removal of Foreign Bodies from the Female Bladder;" *Mr Annandale,* "A Case of Loose Cartilage in a Bursa;" *Dr Chiene,* "On a Case of Intra-Abdominal Hernia;" *Dr J. Bell,* "Case of Compound Fracture of Cranium with Hernia Cerebri and Compound Fracture of Pelvis, followed by Recovery;" and *Dr A. G. Miller,* "Case of Disease of Spine successfully treated with Carbolic Acid." In 1869, *Dr Watson,* "On the Treatment of two cases of Aneurism by Compression;" *also,* "Case of Tumour developed in the Pterygoid Space, and extending into the Buccal Cavity;" *Dr Gillespie,* "Case of Resection of Knee-Joint, with Fibrous Anchylosis;" *Dr Watson,* "Case of Strangulated Femoral Hernia, with Perforation of Intestine;" *Mr Annandale,* "Calculus successfully removed from a Cavity in the Kidney, with Observations on the Operation of Nephrotomy;" *also,* "Case of Strangulated Fatty Hernia;" *Dr Watson,* "Case of Successful Amputation of the Scapula, along with the Clavicle and Remains of the Arm; *also,* "Case of partial Excision of the Scapula for Tumour;" *Dr J. Bell* "Three Cases of Recovery from Acute Pyæmia, with Remarks;" *Dr John Duncan,* "On Galvano-puncture of Nævus;" and *Dr Miller,* "On Polypus of the Rectum." In 1870, *Dr Watson,* "Case of Popliteal Aneurism treated by Compression;" *also,* "Case of Sloughing Hernia;" *also,* "Case of Intra-Cranial Abscess following Injury, successfully treated by Trephining;" *Mr G. R. Gilruth,* "On Cauliflower Excrescences removed from the Penis;" *Dr Chiene,* "Case of Obturator Hernia;" *Dr J. Bell,* "Notes on

Excision of the Mamma;" *Dr A. G. Miller*, "Case of Femoral and Obturator Herniæ;" and *Mr Annandale*, "Case in which an Internal Intestinal Obstruction was removed by the Operation of Gastrotomy." In 1871, *Dr J. Bell*, "Cases of Vesico-Vaginal Fistula;" and *Mr Annandale*, "On the treatment of aggravated cases of Club-foot." In 1872, *Mr Annandale*, "Case of Hæmorrhage resulting from an Internal Perforation of the Œsophagus, treated by Operation;" *Dr J. Bell*, "Case of Ligature of the Subclavian Artery under somewhat unusual circumstances;" *Mr Annandale*, "On Operative Interference in Secondary Growths of Cancer;" and *Dr Cadell*, "The Advantages of Circumcision, from a Surgical Point of View." In 1873, *Mr J. D. Pridie*, "Notes of a Case of Laceration of the Urethra, and Hæmorrhage into the Bladder, from direct Injury;" *Mr G. R. Gilruth*, "Fracture of the Skull, with Puncture of the Brain;" *Dr Watson*, "A New Operation for Anchylosis of the Elbow, resulting from Fracture;" *Mr Annandale*, "On the Operative Treatment of Stricture of the Urethra;" *Dr John Duncan*, "A New Operation for Closure of Artificial Anus;" *Dr Watson*, "Excision of the Thyroid Gland;" *Mr Annandale*, "Operation in a Case of Umbilical Hernia, with Intestinal Obstruction;" and *Dr A. G. Miller*, "Remarks on the Employment of Bulbous Bougies."

At the Conversazione, *Mr Spence* exhibited—"(1.) A Complete Set of Ancient Trepan Instruments, and a Set of Modern Trepan Instruments. (2.) A variety of very ancient blunt and sharp Hooks, Steel Probe, and the 'Great Needle,' so often mentioned in old surgical works. (3.) Ancient Amputating Instruments, including Petit's Tourniquet, of very original form; the Check Artery Forceps that belonged to the Second Monro, almost identical with Amussat's Torsion Forceps. (4.) Modern Amputation Instruments. (5.) A variety of Lithotomy Instruments:—the 'Apparatus Major' of Mariannus Sanctus, A.D. 1550; the Instruments of the Lateral Operation of Frère Jacques, A.D. 1680, including the Compound Dilator of Collot; *Fac simile* of those used by a North-Western Indian Native Operator; Mechanical Adaptations to give Precision to the Prostatic or Deep Incision. (6.) Modern Lithotomy Instruments. (7.) A variety of Specimens of Urinary Calculi removed by Mr Spence. (8.) Instruments for Tight Urethral Stricture, begun in 1843 to be used by Mr Spence. (9.) The Grooved Catheter used for Urethrotomy by Dr Gouley of New York."

Mr Lister exhibited "Specimens of the various Dressings and Appliances used in conducting the Antiseptic System of Treatment in Surgery." He also showed "some surgical instruments, including the Aortic Tourniquet, probe-ended dressing Forceps useful for extracting small Sequestra, etc., Needles for conveying wire sutures in cases of vesico-vaginal fistula, and Forceps intended to serve as porte-aiguilles during the operation." *Messrs J. F. Macfarlan & Co.* also furnished "Carbolic Plaster, Antiseptic Gauze, Antiseptic

Bandages prepared from Gauze, Oiled Silk Protector, Mackintosh Dressing, Boric Lint prepared with Boracic Acid, Catgut Ligature (Carbolized)."

Dr Watson exhibited "(1.) Two highly-tempered steel probe-pointed tapering Catheters. These instruments were introduced into practice some years since by Dr Watson, for the treatment of retention of urine, in cases of tight stricture of the urethra. They are both probe-pointed. One increases gradually in size to a No. 3 of the ordinary catheter scale, the other to No. 7 of the same scale. The one has a curve of an ordinary bougie, and is adapted for cases of stricture situated anteriorly; the other has the same curve as the ordinary sound, and is specially suited to strictures situated posteriorly near the bulb. The special advantage claimed for these instruments is found in their being absolutely rigid, and in their being probe-pointed. (2.) Three forms of Lithotrite and two of Lithotrite Scoop, all modified to fit to one pair of spring-lever forceps, which, by the crushing force, can be applied with smoothness and rapidity in crushing stones of larger size than 1 inch in long diameter. (3.) A Stricture Dilator of the beginning of last century, with a probe-pointed tapering extremity, a bulbous dilatation 2 inches from the end, and a movement in the handle whereby the blades can be separated from each other to the extent of fully ¾ of an inch. (4.) A Wound Retractor, suited specially to tracheotomy or operations for deligation of arteries, and acting spontaneously on the same principle as that of the spring eyelid speculum, but carrying on each blade three blunt hooks."

Dr Joseph Bell exhibited "(1.) Heurteloup's Lithotrite with its remarkable handle. This instrument was fitted either for crushing the stone by a powerful screw, or for breaking it by strokes of a heavy steel hammer. (2.) Civiale's three-branched Lithotriptic Instrument. The branches were intended to catch and hold the stone, while it was then subjected to the boring action of a sort of screw-headed auger, which played in the axis of the instrument, worked by a powerful handle. These two instruments contain the germs of all the late lithotrity instruments."

Dr A. G. Miller showed "(1.) Native-Indian Instruments used in the operations of Lithotomy, Paracentesis, and for Cataract. (2.) Old Lithotomy Knives of peculiar pattern; an old complicated Instrument for operating in Fistula in Ano; two old Dilators for the Vagina and Female Urethra; a Primitive Speculum. (3.) Liston's Dissecting Case; Liston's Forceps for extracting large Calculi through a comparatively small lithotomy wound."

Mr Andrew Davidson, Medical Missionary at Madagascar, exexhibited "Lithotomy Staff, Guide, and Knife for the operation, devised by Mr Davidson, Madagascar, and used by him in the hospital, Antananarivo. The Staff is rectangular, without the acute angle or knee. A gentle curve joins the horizontal to the vertical portion. The horizontal portion is three inches in length, and grooved on its lower aspect. The Guide, which is five and a half inches long, consists of two blades, capable of being separated by a screw, but when approximated forming an instrument somewhat like a female catheter, but straight, with a groove on the right side, to permit the button-pointed extremity of the knife running along, without escaping from it. The Knife is narrow, slightly triangular; the blade three inches long, furnished with a button-point to suit the groove in the guide."

Dr Cadell showed a "French Instrument for Relieving the Trachea of the Pressure of Goître. It acted in a simple, mechanical manner as a lever, the sharp end being inserted into the tumour, a leather pad acting as fulcrum over the upper part of the sternum, and the lower end of the instrument being kept close to the chest by a bandage."

Mr Mackay, Pharmaceutist, exhibited "Prepared and Dried Specimens of Cibotium, portion of Root of a Tree Fern. Has a place in the Dutch Pharmacopœia, and is used as a Styptic by the natives at Sangora. Folia Ficûs Lasiophyllæ. Down off Ficus Lasiophylla—as used to stop Bleeding by the natives of Singapore."

(2.) *Ophthalmic Surgery.*

In 1823, *Mr James Wardrop*, Surgeon Extraordinary to the King, read "An Account of the Exanthematous Ophthalmia, with Observations on its Treatment." In 1824, *Mr Alexander Watson* read "Observations on Chronic Inflammation of the Iris." In 1831, *Dr Abercrombie* read "Remarkable Affection of Vision." In 1835, *Dr John A. Robertson*, "Remarks on the Operations of Extraction and Displacement of the Cataract, accompanied by a Table showing the Relative Success attending these Operations." In 1841, *Dr James Hunter*, "Case of Temporary Amaurosis following the Extraction of a Tooth from the Upper Jaw;" and *Mr William Walker*, "On the Defective Vision of the Albino, a Case from the Notes of the late *Dr James Hunter*, with Remarks." In 1843, *Dr Robert Hamilton*, "Notice of some Recent Suggestions for the Improvement of Ophthalmic Practice—1*st*, On the Particular Times and Seasons at which the Principal Operations on the Eye should be undertaken." In 1844, *Dr Hamilton*, "On the Excision of the Superficial Layers of an Opaque Cornea as a Means of Restoring Vision." In 1847, *Dr Hamilton*, "Case of Imperfect Vision from Irregular Refraction, with Night Blindness, etc., Relieved." In 1850, *Mr Bell*, "Case of Amaurosis and other Symptoms following Blows on the Eye;" Dr *Williamson*, "Aneurism of the Aorta fatal by Rupture, and attended by Contracted Pupil." In 1863, *Mr Walker* "showed to the Society a patient who had suffered from a most intractable Granular Conjunctiva, in whom, as a last resource, he had been induced to try the effect of exciting Gonorrhœal Ophthalmia. This had been followed by very favourable results. The *President* remarked on the questionable morality of such a method of cure." In 1868, *Mr Bell*, "Three Cases of Congenital Cataract in one Family, cured by Operation;" and *Dr Argyll Robertson*, "On an Interesting Series of Eye Symptoms occurring in a Case of Spinal Disease." In 1870, *Dr D. Argyll Robertson*, "Case of Diphtheritic Conjunctivitis;" *also*, "On Albuminuric Retinitis." In 1872, *Dr Argyll Robertson*, "Case of Extreme Staining of the Conjunctiva with Nitrate of Silver;" *also*, "On Tenotomy of the Superior Rectus;" and *Dr W. Laidlaw Purves*, "Snellen's Operation for Strabismus."

At the Conversazione, *Dr Argyll Robertson* exhibited—"(1.) Twelve Ophthalmoscopes, viz., Helmholtz's, Jaeger's, Coccius', Meyerstein's, Liebreich's (stationary), Liebreich's (small), Desmarres', Hasner's, Soleil's, Loring's, Nachet's (simple), Nachet's (binocular); (2.) Perrin's Model of the Human Eye, constructed for ophthalmoscopic examination. The refractive media may be so altered as to represent an emmetropic, myopic, hypermetropic, or astigmatic eye. Metallic cups, painted on their concave surface so as to depict various diseased conditions, are contained in the case, and may in turn be placed in the model and examined by means of an ophthalmoscope. Two metallic diaphragms, with apertures of different diameter, are supplied, so that the eye may represent one with contracted, medium-sized, or dilated pupil. This model was exhibited by means of Liebreich's stationary Ophthalmoscope, and thus the ophthalmoscopic image was readily perceived."

(3.) *Aural Surgery.*

In 1825, *Mr Thomas Buchanan, of Hull*, read a paper "On Syringing the Meatus Auditorius." In 1844, *Dr Warden* gave a "Notice of an Improved Construction of Aural and other Specula, —instruments exhibited." In 1845, *Dr Warden* read, "Sequel to a Former Communication on the Application of Prismatic Reflexion to the Investigation of Disease in the Open Cavities of the Body, with Cases, and Exhibition of Instruments;" *also*, in 1846, "On Inspection of the Membrana Tympani as an Important Means of Diagnosis in Head Affections;" *and again*, in 1847, "Cases of Deafness, illustrating the Utility of the Prismatic Speculum as a Means of Diagnosis." In 1872, *Dr W. Laidlaw Purves*, "On the Treatment of Chronic Otorrhœa by Spiritus Vini Rectificatissimus."

At the Conversazione, *Mr Lister* exhibited "A Hook adapted for Removing Foreign Bodies from the Ear."

Dr Kirk Duncanson showed "An Ear-scoop for removal of Impacted Foreign Bodies from the External Meatus, made on the model of Civiale's Urethral Scoop; a Myringotomy Knife for perforating and trepanning the Membrana Tympani; various Specula Auris; Weber's Ear Illuminator; Gruber's Apparatus for Inflating the Tympanic Cavity, and for the Injection of Medicated Solutions into the Tympanum; several Forms of Artificial Membrana Tympani (as Toynbee's, Yearsley's, etc.); a Specimen of Chloro-Acetic Acid for destroying Granulations, small Polypi, etc.; Instruments for the Removal of Polypi (as Toynbee's Lever-ring Forceps, modification of the ordinary Nasal Polypi Forceps); Eustachian Catheters, Allen's Nasal Pad, and Allen's Modification of Politzer's Ear-douche."

(4.) *Dental Surgery.*

In 1842, *Dr David Hay* read a "Case of Fatal Hæmorrhage following the Extraction of a Tooth." In 1846, *Dr Roberts* exhibited to the Society his "Apparatus for arresting Hæmorrhage after the Extraction of a Tooth." In 1847, *Dr R. Reid* gave "Alveolar Hæmorrhage Compress, constructed by himself—the Instrument and Model exhibited." In 1848, *Mr F. B. Imlach*, "On the Exhibition of Chloroform in Dental Surgery." In 1852, *Dr Roberts*, On the Application of the Actual Cautery for destroying the Dental Nerve by Means of Electricity." In 1855, *Dr John R. Miller, H.E.I.C.S.*, "Case of Hæmorrhagic Diathesis, in which Bleeding from the Gum was arrested only by Extraction of the Tooth in its immediate Vicinity." In 1861, *Dr Roberts* showed "Specimens of the so-called Indian Caustic Root, a fragment of which, he had observed, had apparently destroyed the sensibility of a Decaying Tooth."

At the Converazione, *Dr John Smith* exhibited "(1.) Specimens of Human Teeth, interesting practically and pathologically. (2.) Casts illustrative of the great approximation of the Lateral Parts of the Mouth, in adults affected with Congenital Fissure of the Palate. (3.) A Lower Jaw; being a well-marked example of the Condyles, being, in their articular surface, the thread of a screw passing round an axis at right angles to the antero-posterior plane of the jaw. (4.) Models (nat. size) of the Teeth of the Iguanodon, Megalosaurus, Mastodon, and Machairodus. (5.) Cast of the Roof of the Mouth, in a case where both Upper Maxillæ had been, at different operations, removed. (6.) A number of Teeth, showing the action upon them of various Therapeutical Preparations of Iron."

Dr Roberts exhibited "(1.) A number of Upper Molars, with *four* distinct roots; Under Molars, with *three* distinct roots; also some Bicuspids, with *three* roots. (2.) Cases of Exostosis of the Fangs of Teeth. (3.) *Supernumerary* Teeth, having their distinctive characters. (4.) Two Upper Lateral Incisors *united*, on each side. (5.) Cases of deposit of Tartar on Teeth and Artificial Piece. (6.) Alveolar Compress, to be used in cases of Alveolar Hæmorrhage."

Mr T. W. Hogue showed "Three Casts of Fissured Palates, with the Artificial Vela used in each case in position; an Artificial Palate attached to the nipple of a Feeding Bottle; also the type-metal moulds in which the soft rubber vela are produced. The artificial palates are made of soft vulcanized rubber, specially prepared for the purpose, and the retaining plates of hard elastic gold The principle on which they are constructed, is that first shown by Stearns, and now applied by Kingsley with so much success, viz, 'The rendering available the muscles of the natural palate to control the movements of the artificial palate.'"

Mr Andrew Wilson exhibited "The Vulcanite portion of a Case in which the Right Superior Maxillary and Palate Bones, and also part of the Left Maxillary, had been replaced. It was *hollow*, and made of uncoloured Vulcanite, weighing only 149 grains, while a *solid* duplicate in pink Vulcanite weighed 639 grains—a difference of fully an ounce."

X. MEDICINE.

In 1822, *Dr James S. Combe, Leith,* read "On Anæmia;" and *Dr John Yule,* "Case of the Deleterious Effects produced by the presence of the Larva of an Insect in the Human Stomach." In 1823, *Drs A. Duncan, jun., and William Cullen,* "Cases of Empyema;" *Mr Alexander Watson,* "Case of Abscess of the Lung, with Pneumo-Thorax;" *Dr Louis Frank of Parma,* "Remarks on the Destruction and Expulsion of Teniæ from the Body;" *Dr A. Duncan, jun.,* "Severe and Protracted 'Nervous Disease' in a Medical Man;" and *Dr A. J. Ralph,* "On the Yellow Fever as it occurred in the 2d Regiment of Foot in Barbadoes, in 1816 and 1817." In 1824, *Mr Sherwin, Hull,* "Case of Pompholyx Diutinus;" *Dr John Aitken,* "Cynanche Laryngæa in a Patient aged 75, illustrated by a Preparation;" *Dr Gairdner,* "Case of Dropsy, in which an unusual appearance of enlarged vessels presented itself on the surface of the Abdomen, with a Cast;" and *Dr Duncan, jun.,* "Death from Inflammation of the Gall-Bladder from a Calculus." In 1825, *Dr A. F. Holmes, Montreal,* "Carditis, with Anomalous Symptoms;" *Mr A. Watson,* "Case of Abscess of the Lung, with Pneumo-Thorax;" and *Dr A. H. Renton of Madeira,* "Observation on the Dysentery of Madeira." In 1827, *Dr Thomas Molison,* "Aneurism of Descending Aorta opening into Œsophagus, with Disease of Spinal Cord and Paralysis of Upper Extremities;" and *Mr W. Brown,* "Cases of Cerebral Affections occurring at the close of Chronic Diseases." In 1828, *Dr Dumbreck,* "Emphysema without Local Injury;" and *Mr A. Watson,* "Case (exhibited to the Society) of the peculiar Tubercular Disease of the Skin, called Molluscum." In 1831, *Dr Gregory,* "Three cases of Palsy of the Facial Muscles on one side, connected with different Organic Changes within the Head (parts exhibited);" *Dr Christison,* "Several cases of Partial Paralysis of the Nerves of the Face;" *Dr Graham,* "Enteritis following Pleurisy and the use of Tartar Emetic;" *Dr Allsop, Birmingham,* "Periodical Headache, terminating by Effusion of Serum into the

Ventricles, with a Tumour of Scirrhous hardness in the Cerebellum;" *also*, "Epilepsy terminating in Coma, with no decidedly Diseased Appearances;" and *Dr Gregory*, "Fatal Apoplexy with unusual Symptoms connected with Disease of the Arteries of the Brain." In 1833, *Mr W. Wood*, "Account of the Scarlatina as it lately appeared in Heriot's Hospital;" *Dr Alison*, "Notice of a Succession of Cases of Fatal Peritonitis;" and *Mr W. Brown*, "Notice of the late Influenza." In 1834, *Drs John Gairdner and Beilby*, "Three Anomalous Cases of Spasmodic Affections of the Respiratory Organs;" *Dr Alison*, "Cases of Internal Inflammation terminating rapidly, and supposed to be Erysipelatous;" *Dr John Smith*, "Cases exemplifying the connection between Diseased Heart and Diseased Brain;" and *Dr John Scott*, "Of Apoplexy, with Remarkable Symptoms;" *also*, "On Intermittent Headache." In 1835, *Dr Peebles* (two papers), "On Contagious Petechial Fever;" *Dr Alison*, "Long-continued Constipation, with threatening of Ileus, successfully treated;" *Dr Maclagan*, "Case of Meningitis, with Dissection;" and *Dr Alison*, "Case of Psoriasis terminating fatally." In 1836, *Mr Alex. Cockburn, R.N.*, "Case of Spinal Disease followed by Mania, with recovery from both;" and *Dr Beilby, jun.*, "Disease of the Heart, with Obstruction of several of the Great Vessels by Coagula, apparently formed during life." In 1838, *Dr S. Scott Alison, Tranent*, "Singular Case of Disease of Uterus and Stomach;" and *Dr W. H. Madden, Penicuik*, "Case of Polydipsia." In 1839, *Mr W. Brown*, "On three Fatal Cases of Scarlatina." In 1842, *Dr John Scott*, "Cases and Observations on a Peculiar Affection of the Respiratory Organs;" and *Dr Smith*, "Case of Mania presenting peculiar Symptoms." In 1843, *Dr M'Kinnon*, "On Impulsive Insanity;" and *Dr Henderson*, "On the Characters which distinguish Typhus Fever from the present Epidemic." In 1844, *Dr Scott*, "On a peculiar Affection of the Respiratory Organs in Females;" and *Dr Christison*, "On a particular Class of Cases of Functional Disorder of the Heart, which are apt to be mistaken for Hypertrophy." In 1845, *Dr Paterson, Leith*, "Case of Rupture of the Lung, with Effusion of Blood into the Chest, in connexion with External Injury." In 1846, *Dr Thomas Lee*, "Some Observations on a Fever which has prevailed in Edinburgh during the present year;" *Dr Begbie*, "On Rheumatism and Chorea,—their Relation and Treatment;" and *Dr Peddie*, "Case of Spinal Apoplexy." In 1847, *Dr Christison*, "Sequel to Paper on Scurvy." In 1848, *Dr Sellar*, "On Curable Headache: its Pathology and Treatment;" and *Dr Begbie*, "Anæmia and Goitre, are they related?" In 1849, *Dr R. Paterson*, "Remarkable Case of Shipwreck, with Remarks on the Disease which cut off the only Survivor;" *Dr Taylor, Penrith*, "Case of Morbid Grinding of the Teeth;" *Dr Begbie*, "On Stomach and Nervous Disorders, as connected with the Oxalic Diathesis;" *Dr Lucas of Dalkeith and Dr Christison*, "On Hydrophobia," with Discussion in full, by *Dr W. T. Gairdner, Mr Dick* (Vet. Col.), *Dr J. H. Bennett, and Mr Syme; Mr Sharp, Cullen,* "Case of Intestinal Calculi;" and *Dr W. T. Gairdner,* "Case of Aneurism of the Superior Mesenteric Artery." In 1850, *Dr J. H. Bennett,* "On the Treatment of Phthisis Pulmonalis;" *Dr J. Gairdner,* "On a peculiar Affection of the Nasal Fossa;" *Dr J. Y. Simpson,* "On a peculiar Affection of the Intestines;" *Dr Begbie,* "Remarks on Erythema Nodosum, and its connexion with the Rheumatic Diathesis;" *also,* "On the Climacteric Headache of Females, and its connexion with the Phosphatic Diathesis;" *Mr Charles Sidey,* "Fatal Case of Hydrophobia;" *Dr J. H. Bennett,* "On Leucocythemia;" and *Dr W. T. Gairdner,* "On some Cases of Adherent Pericardium." In 1851, *Dr Begbie,* "Thoughts on Fatty Degeneration of the Heart, chiefly in reference to some points in Diagnosis;" *Dr Alex. Wood,* "On certain Cases of Acute Bright's Disease, simulating Acute Hydrocephalus;" *Dr Imlach, Liverpool,* "Case of Hæmorrhœa Petechialis;" and *Dr W. T. Gairdner,* "Case of undiscovered Aneurism of the Aorta, fatal by Orthopnœa." In 1852, *Mr John Tait, late Madras Army,* "Observations on Hepatic Abscess;" *Dr Alex. Wood,* "On some of the more usual Complications of Scarlatina, and more especially in the relations between Scarlatina and Erysipelas;" *Dr J. W. Begbie,* "On Temporary Albuminuria as occurring in the course of certain Febrile or other Acute Diseases;" *Dr Begbie,* "Illustrations of Erysipelas;" *Dr Keiller,* "Case of Erythema Nodosum;" *Dr J. H. Bennett,* "Illustrations of Laryngæal and Pharyngæal Disease, frequently mistaken for, or associated with, Phthisis Pulmonalis;" and *Dr J. W. Begbie,* "Case of Acute Rheumatism, succeeded by Chorea and Affection of the Heart." In 1853, *Dr W. T. Gairdner,* "Remarks on the Diagnosis and Treatment of Bronchitis, and of the Collateral Acute and Chronic Affections of the Lung;" and *Dr John Struthers,* "Case of Paralysis of the Common Motor Oculi Nerve." In 1854, *Dr Coldstream,* "Case of Catalepsy, with Remarks." In 1855, *Dr W. T. Gairdner,* "On the Differential Diagnosis of Pneumonia and Pleurisy;" *also, Dr Broadbent,* "Case of Tubercular Leprosy—patient exhibited;" and *Dr J. W. Begbie,* "Termination of a Case of Rheumatism and Chorea, formerly submitted to the Society." In 1856, *Dr Fraser Thomson, Perth,* "Cancer of the Lung following Excision of the Mamma for presumed Malignant Disease;" and *Dr David Wilson,* "Case of Rupture of the Spleen." In 1857, *Dr Playfair,* "On Cases of Ruptured Spleen;" *Dr J. W. Begbie,* "Case of Persistent Sarcina in the Urine;" *Dr Laycock,* "Microscopic Observations on a Urinary Deposit in Cases of Rheumatism;" and *Dr J. W. Begbie,* "Aneurism of the Aorta, with Laryngæal Spasm, in which Tracheotomy was performed." In 1859, *Dr W. T. Gairdner,* "On Pericarditis." In 1861, *Dr Bruce,* "Case of Croup in the Adult;" and *Dr J. W. Begbie,* "On Ichthyosis." In 1860, *Dr Skae,* "On the General ralysis of the Insane." In 1863, *Dr Laycock,* "On Protrusion

of the Eyeballs, with Bronchocele and Palpitations;" and *Mr J. B. Hislop, Houston*, "Two Cases of Murrain (Aphtha epizootica) in Man, with Remarks by *Dr George Balfour*." In 1865, *Dr Joseph Bell*, "Case of Paralysis following Diphtheria;" *Dr Francis Skae*, "On Climacteric Insanity;" *Dr Haldane*, "On the Nature and Treatment of Uræmic Convulsions;" and *Dr Sanders*, "On a Case of an unusual form of Nervous Disease, with Remarks." In 1866, *Dr Whiteford, Greenock*, "Case of Addison's Disease." In 1867, *Dr T. G. Stewart*, "Case of Gastritis Phlegmonosa." In 1868, *Mr Bell*, "Case of Extravasation of Blood into the Pericardium, in which the individual survived One Hour;" *Dr Turner, Keith*, "Case of Infantile Convulsions;" *Dr A. H. Douglas*, "Case of Plastic Bronchitis, *quasi* Diphtheritic;" *Dr Douglas Maclagan*, "Case of Sudden Death in Hysteria;" *Dr John Duncan*, "On Herpes Zoster;" and *Drs George Balfour* and *T. G. Stewart*, "Case of Enlarged Spleen, complicated with Ascites." In 1869, *Dr Douglas*, "Abscess of Lung—a case, with Recovery." In 1870, *Mr Bell*, "A peculiar Paralytic Condition of the Lower Extremities following Gastric Fever;" *Dr T. G. Stewart*, "Two Cases of Diphtheritic Paralysis simulating Locomotor Ataxy;" and *Dr Claud Muirhead*, "Notes of Cases of Relapsing Fever." In 1872, *Drs Joseph Bell* and *Halliday Croom*, "Case of Obstruction of the Bowels;" *Dr J. W. Begbie*, "The Swelled Leg of Fevers;" and *Dr T. G. Stewart*, "Notes of a Case of Inflammatory Bright's Disease fatal in the third stage." In 1873, *Dr Stephenson*, "Clinical Observations on Pneumonia in Children;" and *Dr T. J. Maclagan*, "Uræmia and Nervous Symptoms of Fever."

XI. OBSTETRICS.

In 1824, *Dr Buchanan of Hull* read a paper entitled, "Breech Presentation successfully treated without introducing the Hand into the Uterus." In 1825, *Dr Louis Frank of Parma* communicated a "Case of Rupture of the Uterus, in which Gastrotomy was employed with success." In 1826, *Mr W. A. Dalziel, R.N., Wigtown*, gave a "Case of Fœtus expelled *per anum*, after having been retained above five years." In 1829, *Dr William Young*, "Case of Extra-uterine Fœtation, connected with Retroversio Uteri." In 1830, *Mr J. M. Baynham, Birmingham*, "Case of Retroverted Uterus treated by Puncture of that Organ, with Remarks on the circumstances which indicate the employment of that Operation." In 1831, *Mr A. Watson*, "Case of Fatal Hæmorrhage into the Cavity of the Abdomen from the bursting of a small Tumour on the side of the Uterus." In 1834, *Dr John Scott*, "Of Irritable Uterus." In 1835, *Dr Christison*, "Case of Fallopian Pregnancy Fatal by Rupture;" and *Dr John Mackintosh*, "Communication on Dysmenorrhœa, where silver probes of different sizes were introduced into the Os Uteri in cases of Contracted Os and Cervix." In 1837, *Dr J. Simpson*, "Notices of Cases of Peritonitis in the Fœtus in Utero." In 1838, *Mr Charles Sidey*, "Cases illustrative of the Connection between Puerperal Fever and Erysipelas." In 1840, *Dr James Simson*, "Case of Death from Retention of a Catheter in the Female Bladder, for which the usual Operation for Lithotomy was performed;" and *Dr J. Y. Simpson*, "Case of Amputation of the Cervix Uteri, with an attached Cauliflower Excrescence, followed by Pregnancy—preparation shown."

During this year an auspicious event occurred, in *Drs Wm. Beilby, J. Y. Simpson, Alex. Ziegler*, and other zealous members of our Society originating "THE OBSTETRICAL SOCIETY,"[1] an offshoot remarkable for her practical thoroughness and success; yet, as may be seen, in nowise weakening the quality of the sap flowing along the parent stem; many distinguished Members of *both* Societies habitually giving us prominent Papers in Obstetrics. In 1841, *Dr Ladesma of Salamanca* communicated a "Case of Hernia of the Gravid Uterus, in which the Cæsarian Section was successfully performed;" and *Dr Pagan*, "Sequel to a Case of Vesico-Vaginal Fistula, formerly read before the Society." In 1843, *Dr J. Y. Simpson* read, "Proposals to advance the Diagnosis and Treatment of Diseases of the Uterus by the introduction of Instruments into the Cavity of the Organ;" *also*, "On the Influence of the Sex of the Child upon the Dangers and Mortality incident to Parturition." In 1844, *Mr C. Sidey*, "Case of Gestation impeded by an enormous Fibrous Cyst within the Abdomen, relieved by Paracentesis, with Remarks;" *Dr J. Y. Simpson*, "On Mechanical Dilatation of the Os and Cavity of the Cervix Uteri, as a means of Diagnosis and Treatment in some Affections of that Organ;" *also*, "On the Expulsion and Extraction of the Placenta before the Child." In 1845, *Dr J. Y. Simpson*, "Cases of Excision of the Cervix Uteri on account of Fungating Disease;" and *Dr Bennett*, "Observations on Ovarian Dropsy, with a Case in which *Dr P. D. Handyside* had removed both Ovaries, but symptoms of ileus and constriction of the ileum caused Death on the seventieth day; with Remarks by *the latter* on Ovariotomy in general." In 1846, *Dr H. Imlach, Sittingbourne*, "Removal of a Foreign Body from the Uterus." In 1847, *Dr Thomas Lee*, "Observations on Puerperal Fever." In 1849, *Dr J. Y. Simpson*, "On the Air-tractor as a Substitute for the Midwifery Forceps, with experiments to show its power;" *also*, "On the Detection and Removal of Intra-uterine Polypi." In 1850, *Mr Patrick Brown*, "Case of extreme Hæmorrhage accompanying the passage of a Uterine Polypus;" *Dr Graham Weir*, "Case of Successful Extraction of a Child by Cæsarian Section, in the Edinburgh Maternity Hospital;" and *Dr J. Y. Myrtle*, "Tumour for which the Operation of

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[1] With which "*The Anatomical Society*," founded in 1833 by Drs John Reid, J. Y. Simpson, and others of our Members, became now amalgamated.

Ovariotomy was performed more than twenty-five years ago." In 1853, *Dr Charles Wilson*, "Practical Remarks on Breech Presentations." In 1854, *Dr Keiller*, "Case of Hysteria and Spurious Pregnancy;" and *Dr J. Y. Simpson*, "Case of Cæsarian Section." In 1855, *Dr Matthews Duncan*, "On Local Bloodletting in Chronic Metritis." In 1856, *Dr M. Duncan*, "On Deformities of the Pelvis from Rachitis and Malacosteon;" *Dr David Johnstone, Montrose*, "History of a Case of Extra-uterine Pregnancy;" and *Mr A. M. Edwards*, "Case of Ovariotomy." In 1857, *Dr Keiller*, "Cases illustrating the Surgical Treatment of Uterine and Vaginal Prolapsus, depending on Perineal Laceration, etc." In 1859, *Dr M. Duncan*, "The Cervix Uteri in Pregnancy." In 1860, *Dr M. Duncan*, "On a form of Leucorrhœa in Old Women." In 1862, *Dr M. Duncan*, "On a consequence of Imperfect Abortion." In 1863, *Dr M. Duncan*, "On Gingival Diphtheritis in Lying-in Women;" and *Dr Thomas Keith*, "On Ovariotomy, with Cases." In 1864, *Dr Stockwell*, "Case of Vulvar Diphtheria." In 1865, *Dr P. D. Handyside* gave a "Case of extreme Loss of Blood after Abortion, in which he had performed Transfusion with Success, employing simply a *grooved* lancet, along which he slid the syringe-nozzle." In 1866, *Dr Gillespie*, "Case of Removal of a Fibrous Tumour of the Uterus, weighing 29 lbs.;" and *Dr M. Duncan*, "On the Nature, Source, and Surgical Treatment of the Bleeding of Fibrous Uterine Tumour." In 1867, *Dr Tuke*, "Cases illustrative of the Insanity of Pregnancy, Puerperal Mania, and Insanity of Lactation." In 1868, *Mr Annandale*, "Case of Ruptured Perineum with Prolapsus Vaginæ, cured by Operation;" *Dr M. Duncan*, "On Pelvic Areolar Inflammation and Sloughing;" *also*, "On the Adherent and Fixed Uterus;" *also*, "On the Uterus as merely fixed by Adhesions." In 1869, *Dr M. Duncan*, "On the Elongation of the Cervix Uteri after Labour, and in other conditions;" and *Dr Whiteford, Greenock*, "Removal of an Imbedded Uterine Fibroid." In 1870, *Dr R. Paterson*, "On Acute Leucocythæmia in connexion with Pregnancy." In 1871, *Dr Joseph Bell* read his "Cases of Vesico-Vaginal Fistula."

At the Conversazione, *Dr Keiller* contributed "Casts to illustrate the application and use of the Cephalotribe as an Obstetric appliance for Crushing and Extracting the Child's Head in Cases of Pelvic Deformity.

"The *first* Cast shows the instrument as it was applied to the head, and used for the extraction of the detruncated head from the uterus. In this case (that of a woman with a pelvis contracted at the brim to such an extent as made it doubtful whether it was possible for her to bear a living child), the pregnancy had been allowed to go on to the full time, and delivery was attempted by the use of the forceps, and afterwards by turning. By this means the body was withdrawn through the pelvic brim, but it was found impossible to extract the head, which remained *in utero*, the body separating from the head by the force applied to extract the child. The Cephalotribe was now called into requisition to extract the detached head from the uterus, an operation well known to be of considerable difficulty, but for which, as proved by this case, the Cephalotribe is peculiarly applicable.

"The *second* Cast exhibits the application of the Cephalotribe in the next pregnancy of the same patient. Premature labour was in this instance induced between the seventh and eighth months, by dilatation with caoutchouc dilators. As in the previous case, turning was resorted to; but it was subsequently found impossible to withdraw the head through the contracted pelvis without the application of the Cephalotribe, which in this latter case saved the necessity of complete detruncation.

"The *third* Cast shows the application of the Cephalotribe as a Crushing instrument and as an Extractor also.

"The three casts are highly illustrative of the advantage of the Cephalotribe in cases where craniotomy would be considered the proper course of procedure. This advantage is specially to be noticed in the combined result of compression and extraction, thus uniting in a simple and efficient manner the more complicated and tedious instrumental measures of craniotomy and extracting forceps."

Dr M. Duncan exhibited "(1.) A Fœtus of fourth month, much decayed, which was expelled with a living healthy child at the full time. The parent had slipped her foot when four months pregnant; abortion threatened, but did not occur. (2.) Coloured Drawing, magnified, of the Anatomy of the Placenta, made by Mr Turner from original observations. (3.) John Reid's plan of the Anatomy of a Maternal Placental Cell. (4.) Coste's plate showing Decidua in a two months' pregnancy. (5.) Plan of the Decidua according to John Hunter's erroneous views. (6.) Double Monster. Chest and abdomen coalesced; one fœtus much larger than the other. (7.) Double Monster. The two fœtuses united exactly as the Siamese twins. (8.) Specimen of Fœtus Papyraceus. (9.) Lithopædion."

Dr Alexander Simpson showed "Dr Prothero Smith's Case of Exploring Needles and Aspiratory Syringe, for exploration and evacuation of cysts, abscesses, and other fluid collections."

Messrs Duncan, Flockhart, & Co., exhibited "Specimens of some of the forms of Pessaries, Suppositories, and Urethral Bougies, made with Cacao Butter; and Pessaries and Suppositories made with Fusible Gelatine."

(*To be continued.*)

Part Second.

REVIEWS.

The West Riding Lunatic Asylum Medical Reports. Edited by J. CRICHTON BROWNE, M.D., F.R.S.E. Vol. III. London: 1873.

THERE is no question that the third volume of the West Riding Reports is much better than its predecessors. It contains fourteen articles, all of which show evident marks of careful preparation and thoughtful work.

Dr Ferrier's experimental researches in cerebral physiology and pathology excited such a widespread interest from the moment of their first publication, that any remarks which we can now make seem somewhat superfluous. To criticise his experiments it would be necessary to repeat them, and this we have neither the opportunity nor the desire to do. We have not even had the opportunity of witnessing the demonstration which Dr Ferrier has made of some of his experiments, and which have been thought very satisfactory by those who were present. The brain had been subjected to the stimulus of electricity, by several physiologists, without any results worthy of attention being evoked. Onimus and Legros, in their large *Traité d'Electricité Médicale*, published as late as 1872, after detailing some experiments on animals, remark that induced currents, whether applied upon the cortical substance of the hemispheres, or upon the optic thalami and corpora striata, neither bring about any muscular contraction nor any other appreciable phenomenon. Fritsch and Hitzig, however, showed, by passing the continuous current through the brains of dogs, that crossed movements could be excited by irritation of the anterior convolutions.

Dr Ferrier, in his experiments, has exclusively used faradization, and has arrived at a method of inquiry which has already yielded the most striking and valuable results. The application of an electrode conducting a weak current to the brain of animals, stripped of the membranes and partially under the influence of chloroform, has been found to excite muscular motions which accurately correspond to the portion of the brain to which stimulation has been applied. The motor centres of a number of muscular regions have thus been mapped out.

The principal conclusions are, that—

"1. The anterior portions of the cerebral hemispheres are the chief centres of voluntary motion, and the active outward manifestation of intelligence.

"2. The individual convolutions are separate and distinct centres, and in certain definite groups of convolutions (to some extent indicated by the researches of Fritsch and Hitzig), and in corresponding regions of non-convoluted brains are localized the centres for the various movements of the eyelids, the face, the mouth (and tongue), the ear, the neck, the hand, foot, and tail Striking differences corresponding with the habits of the animal are to be found in the differentiation of the centres. Thus the centres for the tail in dogs, the paw in cats, and the lips and mouth in rabbits, are highly differentiated and pronounced.

"3. The action of the hemisphere is in general crossed, but certain movements of the mouth, tongue, and neck are bilaterally co-ordinated from each cerebral hemisphere."

The cerebellum is stated to be the co-ordinating centre for the muscles of the eyeball, as well as to preside over the maintenance of the equilibrium of the body. Dr Ferrier has received from the Royal Society a grant which enabled him to carry out his experiments on monkeys and other animals, and some of his farther researches are quoted in the *Journal of Mental Science* for April 1874:—

"Generally it may be stated that the centres for the movements of the limbs are situated in the convolutions bounding the fissure of Rolando—viz., the ascending parietal convolution with its postero-parietal termination as far back as the parieto-occipital fissure, the ascending frontal, and posterior termination of the superior frontal convolution. Centres for individual movements of the limbs, hands, and feet are differentiated in these convolutions.

"Further, in the ascending frontal convolution, on a level with the posterior termination of the middle frontal, are centres for certain facial muscles—*e.g.*, the zygomatic, etc. At the posterior termination of the inferior frontal convolution and corresponding part of the ascending frontal are the centres for various movements of the mouth and tongue. This is the homologue of 'Broca's convolution.' At the inferior angle of the intraparietal sulcus is the centre for the platysma."

Dr Ferrier's discoveries seem principally confined to those parts of the brain which have motor functions. The cerebral termination of the auditory nerve is believed by him to reside in the superior temporo-sphenoidal convolution. Many parts of the brain yield no results to this kind of irritation. This is the case with the antero-frontal region of the hemispheres, the inferior frontal and orbital convolutions, and those of the central and occipital lobes in the monkey. The optic thalamus, fornix, hippocampus major, and convolutions grouped round it, are stated in the West Riding Report to have no motor signification, and are believed to be probably connected with sensation.

Since the first publication of his views, about a year ago, in the *British Medical Journal*, Dr Ferrier has continued his researches with such zeal and assiduity, that the prosecution of the inquiry would seem to be well-nigh in his own hand; but it would be scarcely desirable, considering the great importance of these results, that his experiments should not be repeated by other physiologists.

It has been objected by MM. Carville and Dupuy that the weak induced currents used by Ferrier diffuse themselves over areas of the brain beyond those in the immediate neighbourhood of the electrode, and that the excitations which Ferrier believes are made upon the convolutions of the hemispheres produced their motor effects by being conducted to the base of the brain. M. Dupuy, in a thesis published on the subject, quoted in the *Gazette Médicale de Paris* for 3d January 1873,[1] affirms that there is not a single part of the cortex of the brain which will, every time when stimulated, lead to the same movements. There is no reason to doubt that the electric stimulus is propagated beyond the surface of the convolutions, and it seems to us difficult to say whether the motor impulse came from the gray matter or from the subjacent white matter. But this is a source of fallacy of which Dr Ferrier was well aware, and which he has tried to guard against by combining vivisection with the use of the induced current. He has also shown that when regions lying near to one another are subjected to faradization, the movements produced are often quite different, which one could not expect to happen if the area of stimulus were diffused and ill defined. The objection of Brown-Séquard, from observations in clinical medicine, seems to us more difficult to meet. On this point Dr Ferrier observes:—"The fact that extensive lesions, particularly if slowly developed, of the hemispheres on one or other side may exist without causing any obvious loss of voluntary movements, does not, to my mind, conflict with the positive results ascertained by the direct rousing of the functions of the various centres by means of electrical stimulation. Sudden lesions of a destructive character are accompanied by hemiplegia, which, though incomplete and usually of transitory duration, confirm by their negative phenomena the positive results of the other method. Fritsch and Hitzig ascertained that destruction of the centres, in which they had localized certain movements of the paw in dogs, caused a partial paralysis of the muscles set in action by galvanization of the same centres."

To our mind, the destruction of a particular part of the brain without any paralysis of the muscles over whose motions it is held to preside, is a powerful argument against the inference that these motions are necessarily dependent upon the convolutions which have disappeared, and clinical observation has furnished numerous instances of such lesions without corresponding loss of motion. Dr Ferrier must either hold that parts of the brain can after a time be educated to take up the function of motor centre to the muscles deprived of their old centre, or give up the view that each hemisphere of the brain exclusively presides over the muscular motions of the opposite side of the body.

Dr Ferrier naturally prefers his direct experiments to the study of lesions, which cannot be isolated or circumscribed; but then pathologists may reply that *his* experiments were made on the brains of animals, and that *their* studies are made directly upon the human brain, and that to infer what takes place in the brain of a man from observations made on that of a monkey, is a process liable to mislead. To show, as some anatomists are very anxious to do, that morphologically the simian encephalon is very like the human one, does not make anything much clearer to our mind. The liker the one brain is proved to be to the other, the more wonderful seems the functional difference between them. Dr Ferrier's observations, however, do not throw much light upon the psychical functions of the brain except by showing that portions of the gray matter which had been supposed to be purely seats of intellectual activity are really centres of muscular motions. It appears that Dr Ferrier combats the theory that the motions excited by stimulation of a certain nervous centre are the result of a mental conception—a notion very likely to arise on reading his paper in the West Riding Reports.

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[1] We are indebted for this reference to the Chicago Journal of Nervous and Mental Diseases, No. I., p. 58.

The subject is very complicated, and it would be most unreasonable to expect everything to be explained at once, and all objections resolved. But unless we are greatly deceived, Dr Ferrier has within the last ten months done more towards an explanation of the functions of the brain than has been done for the last fifty years, and has, with the help of his German colleagues, Fritsch and Hitzig, lighted upon a path of inquiry which will lead to discoveries of the highest interest and importance. In hitting upon his method of research, Dr Ferrier might have been favoured by fortune and accident; in improving it and following it out, he has shown powers of observation and analysis which justify the distinction which he has already gained.

The first article in the volume is on the Convolutions of the Human Brain considered in relation to the Intelligence, by William Turner, M.B., London, Professor of Anatomy, Edinburgh. This is a very instructive report of the state of our knowledge of the physiology of the hemispheres. Professor Turner's remarks are arranged under the heads of Mass and Weight of the Brain, External Configuration, Internal Structure, and Vascular Supply. Mr Turner has collected his facts from a great variety of sources, adding many suggestive remarks, the result of his own well-known labours. In reference to the researches of Dr Ferrier, he writes:— "The relations of the frontal lobes to the voluntary movements might indeed have been predicated from the anatomical connexions which are known to exist between these lobes and the great motor corpora striata. Ferrier, moreover, supports the doctrine that the individual convolutions are separate and distinct centres."

"Since Fritsch and Hitzig's paper was published, Professor Nothnagel of Freiburg has communicated the results of a series of experiments on the physiology of the brain, performed by injecting minute quantities of a concentrated solution of chromic acid into definite portions of the cerebral substance. He urges, as resulting from his experiments, various objections to the view that the psychical functions are strictly localized in definite centres of the cerebral hemispheres, and supports the older view of Flourens and other experimenters, that these functions are bound up in the collective mass of the hemispheres." It may be added that Dr Gudden has made some experiments upon young animals, removing or destroying portions of the brain, and noting the results. The power of recovery is greater than in older animals. He thinks that his experiments show that the centre for voluntary motion is situated in the anterior lobes of the cerebral hemispheres.

It is not easy to find fault with one so exact and circumspect as the Professor, but we are disposed to object to his assertion that—"There can be little doubt that the larger nerve cells are functionally more active, and evolve more energy than do those of smaller size. Though more extended data in support of the anatomical continuity of the nerve cells of the convolutions with the nerve fibres is to be desired, yet on physiological grounds there can be little doubt that each nerve cell has at least one nerve fibre, either directly or indirectly continuous with it, so that multitudes of conducting rods are provided in the large nerve centres, along which the energy evolved by the nerve cells can be transmitted. It would be very difficult indeed to conceive how nerve fibres could fulfil the functions which are universally ascribed to them, if they were not anatomically continuous with nerve cells, as without such connection they would be like strings of knotless threads, or the wires of a telegraph cable dissevered from their central battery."

Mr Turner, we imagine, knows well enough that neither the prolongations of the nerve cells nor the Deiter's processes can be traced in continuous tubes to the spinal cord, nor do we see any necessity for assuming that the existence of a continuous tube is necessary to conduct nervous impressions from the gray matter of the brain to the peripheral nerves. A moment's reflection will show that his analogy of the telegraph wire is of no real assistance to his theory. Electricity simply requires a conductor or conductors lying in contact with one another; the nervous force demands the structural continuity of certain tissues made up of fibres and cells.

Dr J. Crichton Browne has a paper on the use of nitrite of amyl in epilepsy. He has great hopes that this singular medicine may be useful in warding off epileptic fits, if inhaled by the patient when an attack is imminent. Dr Browne lays it down "as an inflexible rule, that in all cases of epilepsy in which loss of consciousness occurs at the commencement of the fit, there is an arrest of the blood-supply to the brain in consequence of spasmodic contraction of the intracranial vessels."

The well-known property possessed by nitrite of amyl of dilating the capillaries might be expected to prevent this sudden contraction, and thus prevent the fit, or at least prevent the loss of consciousness which generally accompanies it, and which Dr Browne believes to be due to the anæmic condition, or stoppage of circulation through the brain. Apparently he holds that the "initial unconsciousness" in the tonic stage of the epileptic attack is due to a different cause from the unconsciousness in the clonic stage, when the face becomes flushed, and the capillaries of the brain probably become dilated. In the apoplectiform stage succeeding the convulsions, insensibility is often prolonged for twenty minutes or half an hour, and it is not likely that any one will assert that in this stage contraction of the capillaries still exists. It would seem, if we are to lay any stress upon Dr Browne's theory of the action of nitrite of amyl, that its beneficial effects should mainly, if not exclusively, be obtained, when given immediately before the coming fit. This can only be done in those cases where there is a premonitory aura of the duration at least of some seconds, an opportunity not only rare in itself, but very likely to pass away without being availed of. Dr Browne has succeeded in collecting some instances where fits were believed to have been averted by the inhalation of nitrite of amyl. The dose is from four to six drops upon a bit of blotting paper held over the nose and mouth. Dr Browne has found great advantage from the same inhalations in treating the status epilepticus; and in the Sussex County Lunatic Asylum Report for 1871, we have an account of some cases where the amyl was tried in the second stage, apparently with the result of shortening it. We have ourselves tried it in the second stage, and believe that in some cases it has actually this effect, though on one occasion a new fit recurred a few minutes after the inhalation. Dr Browne supports the view that nitrite of amyl can prevent the occurrence of an epileptic fit by an experiment upon a rabbit rendered artificially epileptic by Dr Ferrier, through the application of the continuous current to the brain. It was found that during the inhalation of ten drops of amyl (a large dose for a rabbit), the convulsive movements ceased, but that they began again when the effects of the drug had been allowed to pass away. The same effects, however, were found to be produced by a full dose of chloroform. We trust that this is too interesting a subject not to be made the matter of farther study and observation, and much credit is due to Dr Browne for the persevering way in which he has pursued his investigations.

Dr Ferrier's researches were begun in great measure to test the views entertained by Dr Hughlings Jackson about the "discharging" and "destroying lesions" in epilepsy, chorea, and hemiplegia; and the experimental confirmation of his views must be highly gratifying to that physician. Dr Hughlings Jackson has two papers in the volume under review: one on the "Localization of Movements in the Cerebral Hemispheres," the other on the "Anatomical, Physiological, and Pathological Investigation of Epilepsies." We are sorry our space does not allow us to consider these papers, which suggest many questions interesting both in pathology and therapeutics.

Amongst the other contributions, there is one on the "Obscurer Neuroses of Syphilis," by Dr T. Clifford Allbutt, which is worthy of study, especially when brain-syphilis has become an object of special attention. There is also some new information to be gained from Mr Major's "Observations on the Histology of the Brain in the Insane," Mr Galton's "Notes upon the Condition of the Tympanic Membrane in the Insane," and Dr M'Dowall's "Studies on the Power of perceiving Colours possessed by the Insane." All the papers, indeed, have their own merits, and we are sorry we cannot review them in detail.

The Puerperal Diseases: Clinical Lectures delivered at Bellevue Hospital. By FORDYCE BARKER, M.D., Clinical Professor of Midwifery, etc. New York: D. Appleton and Co.: 1874.

THESE are more nearly systematic than clinical lectures. They are copiously illustrated by cases, and this may to some extent justify their title. Whatever may be their quality in this respect, they are good lectures. Dr Fordyce Barker is well known to be a highly intelligent obstetrician, well abreast of the knowledge of his day, and this book abundantly proves it. Dr Barker can scarcely be called an original thinker or worker, but he appreciates good work, has the power of inwardly digesting it; and consequently, having a fine flowing literary style, he is a valuable expounder of puerperal diseases for both old and young. He may be placed near Watson, the typical lecture-writer. His careful observations and profound thought make his opinions and comments valuable and welcome to the profession.

There are, all over the English-speaking part of the world, many physicians who will welcome this book, and to whom it will be truly invaluable, introducing into their minds a great afflux of light and truth. Indeed, we do not know any physician, however educated, who will not find it pleasant and instructive.

There are many things in it which we do not approve, but they are almost all points wherein difference of opinion is quite permissible.

The Student's Guide to Zoology. By ANDREW WILSON. London: J. and A. Churchill.

MR WILSON'S Guide to Zoology is really a good book. Well and clearly written, it offers to the student an excellent digest of the principles on which the science of Zoology is founded. To the non-professional student it will be particularly acceptable, as the absence of technical language, along with the perspicuity of style, combine to render interesting a subject which, in the hands of some authors, is sufficiently tedious. We would especially direct attention to the chapter on the Nature of Life as a model of perspicuity, although we do not altogether coincide with the views held by the author. But while we thus state our opinion that the Guide, so far as the writing is concerned, is well-nigh perfect, we at the same time feel that there is a certain want of originality in Mr Wilson's arrangement of the *matériel.* Upon comparing the work now before us with Dr Alleyne Nicholson's "Introduction to Biology," we are struck with the similarity of treatment of the subject, which is more especially obvious in the earlier chapters of both works. True, Mr Wilson's book is the larger, and contains an amount of physiological information not to be found in Nicholson's, but in the purely zoological portions of both works a striking resemblance is to be observed. We may go even further. Upon referring to the syllabus of a course of lectures on Zoology, the distant echo of which still lingers in our ear, we are struck by the resemblance between the heads of the lectures and the contents of the chapters of this book. In fact, we may, to put it shortly, state that the "Guide" is morphologically identical with, although physiologically distinct from, the other works just referred to. It is not for us to inquire into the explanation of these facts, but while we are aware that the biological mind of any particular period is apt to run in certain grooves, we cannot but think that some light might be thrown on this subject by a careful application of the principles which have gone so far to establish the existence of a great law of community of descent. The work is profusely illustrated, and cannot fail to be appreciated by those entering on the study of zoological science. We commend it heartily to their attention.

The Microscopic Structure and Mode of Formation of Urinary Calculi. By H. VANDYKE CARTER, M.D. Lond. With Illustrations. London: J. and A. Churchill: 1873.

THIS is a very interesting and, as we think, successful attempt to fill up a blank in medical literature. The author has had opportunity, leisure, and scientific zeal, and the product has been some very excellent work on a difficult and little-studied subject. The observations have been made on sixty-nine cases, all from Deccan, Sinde, or Bhooj; and from these a most careful table is constructed, showing the relative frequency and proportions of the various ingredients. Oxalate of lime is very frequent, being found as a constituent in about seventy per cent., and a nucleus in more than thirty-five per cent.

The book itself is so precise and concentrated, that it is not easy to condense it farther. From a summary we extract the following conclusions:—

1. That urinary calculi are not mere precipitates or aggregations of ordinary crystalline and amorphous deposits, held together by mucus.

2. An animal basis is an essential component of urinary calculi. This is probably not always the same, and its only approach to organization is a finely granular striated or fibrillated appearance.

3. Peculiar and characteristic ingredients, which are immediately of physical origin, but hold a position intermediate between pure mineral and organic. To these the name *submorphous* is given by the author.

4. Frequently the stone seems to commence by an accumulation of globular urates. The idea that stone is the result of accident is not sustained by these researches.

The mode of formation of calculi is discussed, and the method, termed by Mr Rainey " molecular coalescence," is fully entered into.

These inquiries render the utility of any attempt to disperse calculi very improbable. The free use of pure drinking-water seems to afford hopes of *prevention* of calculus.

The plates are good and clear.

Revue des Sciences Médicales, en France et à l'Etranger. Recueil trimestrel, analytique critique, et bibliographique. Dirigé par GEORGES HAYEM, Médecin des Hôpitaux. Tome II. No. 2. Paris: 1873.

THIS handsome octavo volume, of upwards of 1000 pages, is judicious in plan, method, and arrangement, is excellently printed in clear bold type, has a most copious index, and very full bibliographical information. It seems to have been selected with great care, and with great impartiality, from all sources; and English-speaking nations cannot complain that they have been neglected, or that their novelties have been unnoticed. In Anatomy, Physiology, and Medical Chemistry, we find German and French authorities chiefly cited; but in Gynæcology and Surgery, we find English and American names occupying a prominent position. We recommend this work not only to medical libraries, but also to all *learned* medical men.

Lectures on Syphilitic Osteitis and Periostitis. By JOHN HAMILTON, Surgeon to the Richmond Hospital, and to Swift's Hospital for Lunatics; late President of the Pathological Society of Dublin. London: J. and A. Churchill: 1874.

THIS well-printed and fully-illustrated little work seems to us to be a most useful one. It treats of a limited but interesting pathological subject, one on which the author has thought much, and which he has had good opportunities of studying. Its information is given in the form of clinical lectures, which are illustrated by cases. The lectures are seven in number. 1. On Syphilitic Osteitis and Periostitis in general. 2. On this disease as it affects the Cervical Vertebræ. Of this comparatively rare form some good cases are given. 3. On the same disease in the Orbit. 4. On the soft Node. In this form we often find cario-necrosis resulting. A good case is recorded and illustrated, in which large portions of diseased bone were removed from the tibia in the vicinity of the ankle-joint, and yet a good cure resulted.

5. *The yellow tubercle.*—By this name our author describes the tertiary symptoms, which the old authors called *gumma.* He recommends removal of these masses by incision and forceps. He does not seem to be aware of the exceedingly good results of blistering in such cases, in hastening the separation of the mass, and promoting healing.

Lecture 6 is on this disease as affecting the Maxillary and Nasal Bones.

Lecture 7 gives treatment in a systematic manner,—Mercury, iodide of potassium, and early removal of dead bone, being the chief lines of treatment recommended.

Die Sensibilitäts-Verhältnisse der Haut. Für die Untersuchung am Krankenbette übersichtlich dargestellt. Von Dr M. BERNHARDT, Privat Docenten an der Universität zu Berlin. Berlin: 1874.

IN this pamphlet we have brought together, in a tabular form, the results of numerous observations by E. H. Weber, Nothnagel, Eulenburg, and the author, to determine the sensibility of the skin in health to different kinds of stimuli.

In the first table, by E. H. Weber, the tactile sensibility of the skin over thirty-nine different parts of the body, is expressed in Paris lines. This is ascertained by noting the *shortest* distance at which the points of a pair of callipers can be felt as a double sensation. For example, the tip of the middle finger is marked 1‴, indicating that the points of the callipers when separated that distance can both be felt, while the dorsal surface of the same finger is marked 3‴, showing that the points of the callipers require to be separated that distance before they are both perceived as distinct. The middle finger, therefore, is three times more sensitive to tactile impressions at its tip than on its dorsal surface. In the second and third tables, by the author, we have the sensibility of the skin over various parts of the body to electric stimulus expressed in centimetres. The sensitiveness of the skin to this kind of stimulus is determined by watching the *greatest* distance to which the secondary coil of an induction apparatus may be removed from the primary without the sensation caused by the current being lost. For example, on the eyelids, the secondary coil may be removed from the primary a distance of 15·7 cm. without the perception of the current being lost, while on the back of the hand the coils require to be approximated 11·6 cm. before the current is felt.

In the fourth table, by Nothnagel, we have the sensibility of the skin to differences of temperature expressed in degrees Centigrade. The results obtained are got by taking two similar wooden cylindrical vessels, the bottoms of which are made of metal, and filling them with water of different temperatures. Each vessel is provided with a thermometer, and placed in succession upon the part to be experimented on. The degree of sensibility is indicated by observing the *smallest* difference of temperature which can be felt. Thus, on the skin of the thigh, a difference of 0·5° C. can be perceived, while on the skin of the back the temperatures require to have a difference of 1·2° C. before they can be recognised as two distinct sensations.

In the fifth table, by Eulenburg, we have the sensibility of the skin to pressure expressed in grammes. The pressure-sensibility is ascertained by means of a special instrument called the *baraesthesiometer*, invented by Eulenburg, and manufactured by Goldschmidt of Berlin. It consists essentially of a spring pressing upon a plate of vulcanite, the degree of pressure exerted being indicated upon a graduated scale with which the spring is connected.

In addition to the tables, we have a lithographic plate of the human figure, marked at numerous points with different coloured numerals and letters corresponding with those given in the tables, and showing at a glance the degree of sensibility to the different stimuli.

It is interesting to observe that the sensibility of one part is not equally great to every kind of stimulus. The tip of the middle finger, for instance, which possesses great tactile sensibility, is not nearly so sensitive to pressure as the skin of the forehead, while the tactile sensibility of the latter is very feeble.

We have much pleasure in commending these tables to the notice of those engaged in the study of the diseases of the nervous system, inasmuch as they furnish trustworthy standards of the sensibility of numerous regions of the body to different stimuli, so that the amounts of the deviations from them in disease, whether in the way of increase or diminution, may be accurately measured.

Die Kittsubstanz auf Reaction des Argentum Nitricum. Mikroskopische und Mikrochemische Untersuchungen. Von Dr Robinski.

In this wordy pamphlet, Robinski shows, what we imagine was already pretty well known, that salts of silver stain all animal tissues alike, and that the "cement substance,"—which, according to Kühne, connects the epithelial cells, plain muscular fibres, and other tissues, and which, according to him, possesses the remarkable and characteristic property of being blackened with salts of silver,—does not exist.

Medical Lexicon: A Dictionary of Medical Science, containing a Concise Explanation of the various Subjects and Terms of Anatomy, Physiology, Pathology, Hygiene, Therapeutics, Medical Chemistry, Pharmacology, Pharmacy, Surgery, Obstetrics, Medical Jurisprudence, and Dentistry; Notices of Climate and of Mineral Waters; Formulæ for Officinal, Empirical, and Dietetic Preparations; with the Accentuation and Etymology of the Terms, and the French and other Synonyms. By ROBLEY DUNGLISON, M.D., LL.D. A new Edition, enlarged and thoroughly revised, by RICHARD J. DUNGLISON, M.D. Pp. 1131. London: J. and A. Churchill: 1874.

THIS title, ambitious and exhaustive as it is, simply and truthfully describes this most valuable and learned work, which briefly but distinctly gives any one who consults it, in the most accessible form, a vast amount of information on medical terminology of all kinds. It has already been well known to the profession for forty years, and this new edition, by the son of the original author, has been well brought up to date. The labour of revision, as well as of addition of new terms, must have been very great, but it has been admirably done. We recommend it in the strongest terms to all who have much to do with medical reference.

On the Modified Turkish and Vapour Bath, and its Value in certain Diseases of the Skin. By J. L. MILTON.

THE title describes very well what we meet with in this book. Mr Milton's tone is moderate, sensible, and, therefore, forcible. He gives only his own experience, and does not venture beyond what he is well acquainted with. The apparatus he describes is very ingenious, and apparently well adapted to its purpose.

Mr Milton has his hobby, apparently, and rides it. He recommends the vapour-bath in a great many skin diseases,—all those in which the eruption is dry, because it causes not only a "watery exudation," but also a "sebaceous secretion of a proper quality."—(P. 9.)

There is nothing about the treatment of syphilitic eruptions by mercurial vapour; but perhaps Mr Milton agrees with us in thinking that mercurial baths and mercurial inunction are troublesome and dirty methods of applying a remedy which can be better administered otherwise.

It will be seen from what we have said, that the amount of information contained in this little book is very limited; but if any practitioner should be desirous of knowing how to apply a vapour-bath easily, safely, and thoroughly, he will find the necessary information supplied by Mr Milton's pamphlet.

The Science and Practice of Medicine in Relation to Mind, the Pathology of Nerve Centres, and the Jurisprudence of Insanity: Being a Course of Lectures delivered in Guy's Hospital. By J. THOMPSON DICKSON, M.A., M.B. (Cantab.), etc. H. K. Lewis.

THERE is no special subject on which more is written at present than insanity. Not to speak of the numerous papers in the journals or the monographs, the works of Maudsley, Griesinger, Sankey, Laycock, Blandford, Sheppard, and the third edition of Tuke and Bucknill succeeded each other in rapid succession. Now we have a systematic work on the same subject by Dr Thompson Dickson. This appears under melancholy circumstances. The author, a thoughtful and hard-working London physician, was found dead in his carriage one day, just as this his first literary effort of any importance had been printed and was about to be published. The readers and the reviewer of such a book cannot but feel the sadness of the circumstances of its publication.

The increasing interest in the study of the higher functions of the brain and the vast importance of their derangements fully account for the number of recent books on insanity; and the enormous advances that are year by year taking place in our knowledge of the normal functions of the brain will certainly ensure that the number of such books will be as great in the future as they have been of late, for a few years make a book of little value. The range of the field is gradually widening, too, for it is becoming more and more clearly perceived that insanity is merely the chief of a series of brain states that affect most deeply the capacities and morals, the destinies and the diseases, of mankind. Dr Dickson, no doubt, felt this when he chose the title for his book. But its title covers a much wider area than its contents. It is in reality a course of lectures to students on the subject of insanity, didactic in its style, very dogmatic in its opinions, and full of new and fresh illustrative cases of the diseases and conditions he is treating of, drawn from the author's own experience. The chief fault of the book is its intense dogmatism. To give students the idea that all the questions relating to the nature and treatment of insanity are pretty well settled is overdoing the infallibility and omniscience that are required to be exhibited by the lecturer to students. The subject is one that may be made more interesting, and far more suggestive and profitable to advanced, thoughtful students, by a wide and truthful handling, pointing out the infinite number of as yet unsolved questions. This will produce far more inquirers and original workers than the other plan, and that should be one grand aim of the teacher of all nervous diseases, in the present state of our knowledge of them.

After an introductory chapter, and one on the relation of lunatics as to law, Dr Dickson describes the usually received varieties of insanity, giving the symptoms, treatment, pathology, etc., and concludes with two chapters,—the one on the etiology of insanity, and the other on "morbid states of mind." There are some very beautifully executed plates of the microscopic appearances found in the cord and brain in some cases examined by the author; and, scattered through the book, some hideous and useless photographs of patients who were suffering from different kinds of mental derangement.

The way in which Dr Dickson dogmatizes is seen when he asks the question,—"What then is the pathology of acute mania? I think that we have ground to believe that it is cerebral congestion, and in support of this view we have some evidence." Now, we never could understand how it is possible for any person who has thought much about the matter, and takes into account the facts and the laws of action of the nervous system so far as we know them, to believe that the congested state of the vessels in acute mania and other conditions of insanity are the cause of the mental and other symptoms present. If this is not a case in which the morbid action results from perverted cell-function, and from that alone, in its origin and whole course, all the other morbid conditions, such as congestion of the vessels, being secondary, then there is no truth whatever in the doctrine of cellular pathology.

The chapter on Epilepsy and Mania is very good, this being a subject to which the author had paid special attention. He looked on convulsion as being a very secondary phenomenon of epilepsy. His theory is, that this disease is a contraction of the cerebral capillaries and small arterial vessels, the order of its stages being—(1) irritation of the brain; (2) contraction of its vessels; (3) cerebral anæmia, and consequent loss of consciousness. The subsequent muscular contraction he considered, not as irregular explosions of nerve-energy, according to Hughlings-Jackson's now generally received theory, but as a recurrence to the natural tendency of muscle when all the central nerve-control is removed from it. He gives a number of very instructive cases of unconscious epileptic acts, or epileptic insanity. It is no doubt one of the most important of medico-legal questions truly to diagnose those cases, and to distinguish their true character. We knew an epileptic in an asylum lately, who took off his stockings, putting a stone in the toe of each, concealing one in his coat sleeve, and giving the other to another epileptic, the intention being to kill the doctor when he came round. Next day, he was totally unconscious of what he had done, and would not believe it.

The varieties of insanity connected with the puerperal state—menstruation, phthisis, syphilis, gout, rheumatism, and fever—are then described. Dr Dickson by no means accepted Dr Skae's classification of insanity; but he, like all recent writers on the subject, has been compelled to acknowledge the essential connexion of insanity with those bodily diseases and conditions, a connexion that was first brought into prominence by Skae.

In speaking of the treatment of mania, the author says of alcohol

—"You will probably hear very many objections raised against alcohol. The patient's friends will object to its use, upon the idea that it will excite the patient still more, or that it will feed the patient's craving for stimulants; or perhaps the patient will object, and declare that it causes headache; nevertheless, persevere—the headache and the excitement are the first points of weakness which the alcohol will enable you to combat; and if the first dose does not calm the patient, give more."

In speaking of the treatment of melancholia, he says, "Dr Blandford seems to regard opium as exciting a healing influence; and he has found it particularly of service in sub-acute melancholia. The observation does not accord with my own experience; any temporary relief which opium may afford to the melancholic patient is, I believe, always followed by increased depression; and I am convinced that ultimately, in all cases, it is injurious."

The author seemed to think that the anatomical starting-point of general paralysis is to be found in the sympathetic ganglia; and he makes this statement on the faith of some observations by Drs Poincarè and Bonnet as to pigment and fat deposit in the cells of those ganglia. Nothing can show how much of doubt there is still hanging about this *questio vexata* than such an unsupported theory. Altogether, the chapters on General Paralysis are not satisfactory. In regard to it, a writer may well be dogmatic on many points. On these, Dr Dickson was in doubt; while, in regard to some of the really doubtful points, he pronounced an unhesitating opinion. The question of whether the disease is a real pathological entity *per se*, that of its necessary connexion with mental impairment, belong to the former class; and Westphal's distinction between the so-called tabic and paralytic forms of the disease, and the supposed rapidly fatal prognosis of the cases where the pulse falls to 60 or 50, belong to the latter.

In speaking of the etiology of the disease, he mentions a case in which he had observed dementia to follow the administration of chloroform. He wisely sums up the treatment of insanity in the words, "Rest and nourishment." His observations on morbid states of mind are judicious though desultory. The classifications he gives (for he gives no less than three, more or less original) undoubtedly do not fulfil any very good purpose, practically or scientifically. A few sound and practical observations on moral insanity, with some instructive cases of the disease, conclude the book.

The volume is the result of the present most useful movement in the direction of teaching mental disease to all medical students. Even the smaller medical schools in England provide lectures on the subject now; and no doubt such a course must soon become a compulsory part of a medical student's education. The knowledge and experience acquired by a student who had listened to Dr Dickson's lectures we have been noticing, and had attended a proper clinique at the same time, could not fail to be most serviceable to him when he entered on practice.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING VI.

Wednesday, 1*st April.*—Dr RUTHERFORD HALDANE, *President, in the Chair.*

Exhibition of PATHOLOGICAL SPECIMENS, etc. :—

I. *Dr Joseph Bell* showed a CHILD, æt. 2 years, which was under his care for the following remarkable CONGENITAL DEFORMITIES. Both hands, which were strong and muscular, on well-formed arms, were as if cleft into two nearly equal and opposing halves, each half consisting of two digits, which, however, varied in relative size, some being gigantic, others less than normal size. The child could use the hands freely and strongly. The cleft seemed to pass into the metacarpus in both. The left lower extremity is large and powerful, and perfect, with the exception that the second digit is small and shorter than the others, apparently from the non-development of the distal phalanx. The right limb has a perfect thigh and patella, but the leg is small, and bent at an acute angle; it contains only a tibia, the lower end of which projects under the skin, the astragalus being completely dislocated off it, and drawn back by the action of the calf-muscles on a narrow foot, which apparently contains only one representative of tarsus, one metatarsal bone, and one digit. The foot seems perfectly hopeless; but the parents being against any operative interference, Dr Bell was getting a socket made for the limb to enable the little patient to walk. He mentioned that he had lately seen a case very similar to the above, but in which the only perfect limb was the right upper extremity.

II. *Dr P. H. Watson* then showed—(1.) Specimen of FRACTURE OF THE SIXTH DORSAL VERTEBRA, with nipping of the cord and myelitis, obtained from a man who had fallen three storeys. On admission he was unconscious, and paralyzed from the waist downwards, with priapism. Reflex action took place. The priapism and paralysis of sensibility disappeared a week after the accident. The patient survived the accident for four weeks.

(2.) A specimen of FUNGATING SARCOMA OF THE SCALP, with numerous pea-like tumours occupying the parietal region. The pea-like swellings were congenital, the sarcoma a reproduction of a similar tumour which had been operated upon elsewhere some time before.

(3.) SCAPHOID, CUBOID, THREE CUNEIFORM BONES, AND BASE OF THE METATARSAL BONES removed last week in a lad, æt. 19, by Dr Watson's new operation of excision of the tarsus.

(4.) FOOT AND EXTREMITIES OF TIBIA AND FIBULA removed by Pirogoff's amputation. The only difference in this, as in other similar cases on which Dr Watson had performed this operation, was the rapidity of healing as compared with the ordinary amputation at the ankle-joint.

(5.) A MULTIPLE MOLLUSCOUS TUMOUR from the FOREHEAD, closely simulating a cavernous venous nævus, with phlebolites.

(6.) Parts obtained in excision of the end of the HUMERUS in a case of rigidity of the elbow-joint, resulting from accident, followed by rheumatic arthritis.

(7.) A MUCOUS GROWTH from the posterior border of the septum narium from an infant suffering from suffocative pharyngeal breathing, removed through the mouth by excision by a pair of curved scissors.

(8.) CASE OF AMPUTATION AT THE ANKLE-JOINT, on account of a cancerous affection of the plantar surface of the heel. The operation was performed by triple flaps. The lines of incision were shown, and the fact that the uncovered stump could bear the patient's weight in walking without the intervention of any succedaneum.

(9.) PATIENT in whom, six months ago, Dr Watson had performed his new operation of excision of the anterior tarsus and metatarsus. The patient walked briskly about upon her bare foot.

III. *Dr Taylor* then read his CASE OF POISONING BY THE INHALATION OF COAL-GAS, which will appear in a subsequent number of this Journal.

The President remarked on the interest attaching, not only to Dr Taylor's case, which had been so carefully observed, but to the other cases he had collected, and to his observations on the dangers resulting from even trifling escapes of gas, and on the important bearing of this subject on public health.

Dr P. H. Watson, after complimenting Dr Taylor on the value of his cases and his observations, said that he was much interested in the subject, from the fact that, about three weeks ago, his coachman, and his wife and family, were nearly poisoned by an escape of gas into a drain, and from it into the house, which was supposed to be quite protected by traps. It was remarkable, from the distance the gas came along the drain, for the opening was in Queensferry Street, where a pipe had been injured in some drainage works, and his coachman's house was behind Charlotte Square. The family had to sleep for some days with all the windows open to avoid suffocation. Dr Taylor had told us of many risks and dangers from the use of gas, but all these had been stated with great force by the tallow-chandlers, and other vested interests, when gas was first introduced, and he feared that the public would not readily forego the advantages and conveniences of gas, either by turning it off at the main, or dispensing with it in bedrooms. Even flexible tubes, which had their disadvantages, were very convenient for bringing light near. Dr Watson then asked Dr Taylor why oxygen had not been used in the attempt to resuscitate his patient; for if exposure to the atmospheric air had been able to eliminate the poison to so great an extent, surely oxygen would have done it more rapidly, and a supply might easily be obtained.

Dr Ballantyne (Dalkeith) said that he had, some time ago, in London, seen three cases of poisoning by coal-gas, which were treated by Dr Sieveking by means of inhalations of oxygen. All three recovered.

Dr Taylor mentioned that the use of oxygen had been suggested by Dr Coghill in the treatment of his case, but that Professor Maclagan had thought that the fresh air from the sea, in a fine day in June, through the open window, was likely to prove more useful.

IV. *Dr P. H. Watson* then read his paper, entitled, EXCISION OF THE ANTERIOR TARSUS AND BASE OF THE METATARSUS—A NEW OPERATION, which appears in this number of the Journal, page 961.

The President said that the operation seemed to be an improvement, and to leave a useful foot. He called upon the surgeons present to express their opinions.

Dr Joseph Bell said Mr Erichsen had lately expressed his opinion that operative surgery had attained its fullest development in extent —that surgeons had now amputated and excised every possible portion of the body, and tied every possible vessel. While agreeing with this, he believed that progress was still to be made in the direction of saving portions of the body, which are sometimes unnecessarily sacrificed; that this operation by Dr Watson was like the one on the elbow-joint he lately described to the Society—a valuable contribution in this direction; that surgeons were gradually learning that there are many cases, even of disease in tarsus, in which partial operations were advisable. That such partial operations were possible and successful in their results, he believed was due in great measure to two causes: first, that surgeons now, instead of wasting their time, and enlarging their wounds by seeking for and tying all the vessels in these cases, stopped the bleeding by carefully plugging the wound and firmly bandaging the limb, before ever they loosened the tourniquet. He had treated a great many cases of excision and necrosis in this way, without ever having had secondary hæmorrhage; and, secondly, from the care we now took not to allow the external wounds to close at all till the deep parts of the wound were soundly healed.

Dr Taylor said that, in a recent visit to St Thomas's Hospital, he had seen a case under treatment similar to those described by Dr Watson, and, in a bed in the same ward, another patient lying in a "Pat. Watson" splint.

Dr Watson, in reply, described another case which was under his care at present, in which he had excised the ends of the tibia and fibula, all the bones of the tarsus, except the os calcis, and the whole base of the metatarsus. The case was doing well.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING IX.

Wednesday, 11*th March*.—Dr J. MATTHEWS DUNCAN, President, in the Chair.

I. *Dr M'Rae* read a paper on the USE OF THE ELECTRO-GALVANIC CURRENT IN LABOUR, with cases. He also exhibited the machine and other apparatus employed by him, and which were pronounced by various Fellows to be at once ingenious and apparently well adapted to the end in view. This paper appears in the present number of this Journal, at page 1001.

II. *Dr Angus Macdonald* read a paper on a case of RECURRENT PECULIAR TRANSVERSE PRESENTATION, apparently due to malformation of the uterus, which will appear in an early number of this Journal.

Dr Gordon said that doubtless malpresentations often were due to malformation either of the uterus or pelvis. He was of opinion, also, that the fœtus assumed its position early, and maintained this same position to full term.

Dr Duncan said, experience had shown that, contrary to what had been taught, it was frequently most advantageous to seize the nearest leg in cases of podalic version; and if plausible grounds could be advanced for the seizure of the furthest-off leg, equally plausible grounds could be given for preferring the nearest one. He might remark further—and it was an interesting fact—that turning was easy when the uterus was much flattened. When the latter was much elongated, it was more difficult. In reference to the *stellung* or position of the fœtus, he (Dr Duncan) had made some observations, but these were not numerous enough to be decisive; still, they sufficiently confirmed Schultze's observations on fœtal tossing about and changing of position prior to the final presentation.

Dr Milne believed that not a few cases of abnormal presentation were due to an altered shape of the uterus arising from pressure by contiguous viscera which had become diseased. He was not aware that this displacing cause had been sufficiently attended to. He had a lady with a large and floating (left) kidney, and where the presentations were always more or less transverse, evidently from the vagrant viscus altering the shape of the uterus. In like manner, enlargements of the spleen would give rise to abnormal presentations.

SESSION XXXIII.—MEETING X.

Wednesday, 25*th March*.—Dr J. MATTHEWS DUNCAN, President, in the Chair.

I. CASE OF FIBROID OF THE UTERUS, COMPLICATING PREGNANCY AND PROVING FATAL BY TORSION OF THE PEDICLE. BY DR CAPPIE.

This paper will appear in a subsequent number of this Journal.

Dr Wilson said the case was one of much interest, both as regarded the cause of death and the apparent rapid growth of the tumour.

Dr Duncan said that Dr Cappie's case was an interesting one, and in various respects remarkable. There was no doubt as to the nature of the case. It was a globular uterine fibroid attached to the fundus by a pedicle of smaller size than usual, it being not much thicker than the little finger. Such tumours were sometimes difficult to diagnose, especially if fibro-cystic; because the tumour may separate a good deal from the uterus, and thus lead to confusion. As regards the twisting of the pedicle, which was carefully ascertained, it was difficult to settle the time of its occurrence. Dr Cappie had given a quite natural explanation when he dated it from the time the woman fell, and credited it to that accident. However, if the pedicle had been twisted at the time the female fell, it was rather difficult to understand why the severe symptoms, the putrefaction of the tumour, etc., had been so long delayed. These symptoms might perhaps be better explained by noticing what was verified at the post-mortem examination. There was not only torsion, but considerable tension of the pedicle, by the descent of the uterus after its evacuation. This caused a dragging on the pedicle, and helped to complete its strangulation. In reference to the diagnosis, he (Dr Duncan) could not, any more than Dr Cappie, have come a bit nearer to the mark. Referring to torsions, he said that this subject had received a deal of attention of late years. There were torsions not only of the various viscera, but of the uterus itself. In the gravid uterus the left upper angle is found generally to go forward naturally. In the lower animals, such as the cow, the entire uterus will at times become twisted, even on to strangulation. There are many cases of the twisting of ovarian and fibrous tumours connected with pregnancy on record, but we have had no proper explanation of the mechanism. Turner, Rokitansky, and others, had all written on the subject.

Dr M'Rae had reported a case of fibrous tumour of the uterus in the *Lancet* some years ago, where the growth was expelled, and protruded from the vulva, by pains like those of true labour. There was a good deal of hæmorrhage. The woman was seized with the pains when working in the fields. Dr M'Rae ligatured the pedicle, and incised below the ligature, at the same time applying the actual cautery to the stump. The woman made a good recovery.

Dr Underhill thought the sudden enlargement of the tumour due to stasis, arising from the torsion.

Dr Gordon, referring to twistings, related the case of a man who jumped suddenly out of bed, and thereby caused torsion of the small bowel, which proved fatal.

II. CASE OF HYDROCYSTIS CAPITIS. BY DR M'RAE.

Dr Cairns said that anything practical in Dr M'Rae's paper had been damaged by too much of a theoretical character. There were only two kinds of tumours of the head worth naming—the caput succedaneum and cephalhæmatoma. He strongly condemned the practice of interfering with them, unless inflammation or suppuration took place.

Dr Gordon had used the exploring needle with benefit in such cases.

Dr Thomson had seen a great many of these tumours, and in some of them at times the pathology may have been a little obscure. He was disposed to think that Dr M'Rae's case was one of cephalhæmatoma, from the fact of there being the elevated ridge simulating osseous deposit. While some of these tumours might be left alone, others should be punctured; that was to say, when the pressure or irritation was such as to occasion convulsions. He had met with a case (particulars of which he had given to Dr Duncan at the time) where the convulsions had continued for days, and were arrested at once by puncturing. He thought Dr M'Rae might have done so safely in his case. Dr Thomson had punctured in hydrocephalic cases; but these were not encouraging. In cephalhæmatoma, however, he had no hesitation in doing so, in such a case as just mentioned.

Dr Macdonald said that Dr M'Rae had recorded his case with much care and great accuracy, but he could not view his pathology as correct. He was clearly of opinion the case was one of cephalhæmatoma. Dr M'Rae was wrong in calling the swelling a cystic tumour, seeing that it disappeared spontaneously. He saw no reason to abstain from puncturing, especially if done under antiseptic protection, though he had never met with a case in which he judged puncturing necessary. He had seen many of these tumours, and thought them most liable to occur in occipito-posterior cases, where there was extreme internal pressure exerted on the unprotected portion of the child's head.

Dr Underhill said that there was no need to interfere with these tumours, unless convulsions or other evil consequences followed, as they passed away in time. He could not understand how they could produce pressure on the brain, except by pressing over a fontanelle.

Dr Duncan thought there was rarely occasion to interfere with these swellings. However, were mischief occasioned by them, he would not hesitate to evacuate them.

Dr M'Rae replied. He said that he believed his tumour was different from a cephalhæmatoma; it was, in fact, a bladder of serous fluid, being transparent as a hydrocele, and on percussion there was a counter-wave. He had therefore called it Hydrocystis capitis. It should be remembered that, unlike the cephalhæmatoma, this tumour came on gradually, and increased considerably, before it began to diminish and disappear.

III. *Professor Walley* read a paper on the COMPARATIVE ANA-

TOMY OF THE ARTERIAL CEREBRAL CIRCULATION IN ANIMALS AND THE HUMAN SUBJECT, AS BEARING ON PARTURIENT APOPLEXY AND CONVULSIONS, and showed illustrative preparation and diagram, which will appear in a subsequent number of this Journal.

Dr Wilson said they were all much indebted to Professor Walley for his excellent paper. Many cases of convulsions were inexplicable; others were referable to uræmia. As regards the congestive view, that could hardly explain those where the convulsions came on prior to labour.

Dr Hammond could not see how there should be increased pressure of blood in the cerebral vessels during labour.

Dr M'Rae was not aware that apoplexy was so common in parturient women as Professor Walley had represented. He had not met with any cases in thirteen years' practice.

Dr Thomson said the Society was greatly indebted to Professor Walley for his paper. Personally he was much interested in the subject. The dissection and diagrams, too, helped greatly to illustrate the subject, and to recall and revive the anatomy of the older members. Dr Thomson wished to say very emphatically, that while albuminuria explained many cases of eclampsia, there were other cases of convulsions clearly of an apoplectic character, and where speedy delivery arrested these. He had met with such, and usually the women were stout and plethoric. We were too apt nowadays to view these cases as only due to uræmia.

Dr Macdonald said that Professor Walley had not in his opinion proved that blood-pressure would account for the greater frequency of puerperal convulsions in cows. So far as he could judge, the conditions of vascular distribution seemed as favourable to exalted cerebral tension in the mare as in the cow. He would suggest, as Professor Walley had said that convulsions occurred most frequently in fat cattle, that fatty degeneration of the arteries might have much to do with the matter. As a matter of fact, however, these convulsions were only in a small proportion of cases, occurring in women, truly of an apoplectic nature. It was undoubtedly established that many of those cases, formerly said to be serous apoplexy, were just instances of albuminuria or uræmic poison.

Mr Walley said, that however fat, a horse became, its arteries did not grow atheromatous.

Dr Underhill was unable, even by the aid of Professor Walley's dissection and diagram, to see how there should be greater chance of pressure in the cow than in the horse. The difference in the distribution of the arteries, he thought, was quite insufficient to explain it.

Dr Duncan viewed Professor Walley's paper as a valuable contribution in comparative anatomy, and as helping to elucidate the subject of convulsions. He was afraid we were too apt to view puerperal convulsions as uræmic, overlooking the fact that many of them were apoplectic. We might also have the symptoms of apoplexy along with epileptic convulsions. The question of blood-pressure was worthy of attention. Rosenstein and others had investigated the subject, but there was still much to be done. He begged to convey the thanks of the Society to Professor Walley for his interesting contribution.

IV. *Professor Walley* exhibited the DEFORMED FŒTUS OF A COW (ECTOPIA). The case, he said, occurred in the practice of Mr Pollock of Coatbridge. When called upon to examine the cow, he found four legs and the head of a fœtus presenting, and immediately arrived at the conclusion that he had to deal with twins; but after vain efforts to force back one pair of legs into the uterus, and by careful manipulation, he discovered that the whole of the legs belonged to one fœtus, and decided upon delivering by traction, which he accomplished after two hours' hard work, a portion of the lower jaw of the fœtus being torn away in the efforts made to remove it from the uterus. After removal, Mr Pollock at once saw the character of the malformation with which he had been contending, and kindly forwarded the fœtus to me, with the expressed wish that I would examine it,. and give him my opinion upon the nature of the deformity.

The fœtus was alive when Mr Pollock first examined it.

Dissection revealed a healthy, mature male fœtus, the limbs and head being quite natural; the vertebral column formed a sigmoid flexure, being perfectly twisted upon itself, and the whole of the viscera and vessels of supply (thoracic and abdominal) were situated outside the body, but well developed, with the exception of the urethra and anus being imperforate, and the meconium collected *en masse* in the colon. The thoracic surface of the ribs, which were straight, was exposed, and the fœtus could be doubled backwards upon itself; this, in fact, having taken place *in utero*, and thus accounting for the position in which Mr Pollock found it.

The edges of the skin in the central line of the abdomen, through the whole extent of the fissure, were rounded, smooth, and firm; thus showing that no section had been made through it, either during or after delivery. The width from stifle to stifle, and which presented the great obstruction to the extraction of the fœtus, was 18 inches.

The most interesting question in connexion with the subject is, How, at what period of utero-gestation, did the twist take place? That it must have been in existence for a considerable length of time, is proved by the rigidity of the vertebral column and the rounded edges of the fissured skin.

Part Fourth.

PERISCOPE.

REPORT ON OPHTHALMOLOGY.

BY DR ARGYLL ROBERTSON.

GELATINOUS EXUDATION INTO ANTERIOR CHAMBER.

Dr Gunning of Amsterdam relates three cases of iritis, in which a peculiar jelly-like exudation occurred into the anterior chamber. In two out of the three cases the patients suffered from syphilis, while in the third, the presence of that disease was suspected.—*Klin. Monatsbl. für Augenheilk.*, 1872.

POWER OF ACCOMMODATION AFTER CATARACT-EXTRACTION.

Professor Förster of Breslau maintains strongly the view, that after cataract-extraction a certain power of accommodation still remains. From a careful examination of a number of cases in persons of different ages, he found the accommodation in youthful patients (who had been operated on for cataract) materially greater than it is normally in old persons. He also found the relation between extent of accommodation and age of the patient to exist exactly as in the normal eye. A table, embracing the results of his examination of 22 patients after cataract extraction, of ages ranging from 11 to 74, is appended to his paper.—*Klin. Monatsbl. für Augenheilk.*, 1872.

PROFESSOR DONDERS ON APPARENT ACCOMMODATION IN APHAKIA (EYES DESTITUTE OF CRYSTALLINE LENS).

An able paper by Professor Donders, pointing out the fallacy of the view that accommodation exists in aphakia. He refers to Thomas Young, who in 1800 proved that there was no increased curvature of cornea, nor elongation of the optic axis, when near objects were looked at; and who, consequently, placed the seat of accommodation in the lens. Young also, by careful examination of patients, from whom the lens had been removed (taking care to avoid all sources of fallacy), came to the conclusion that in them all power of accommodation was lost, overturning the view propounded by Ramsden and Home, that accommodative power might be retained after removal of the lens. In 1852, the alteration in the curvature of the lens during accommodation was visibly demonstrated, and the only question remaining was, whether this was the only change that occurred during the act of accommodation. To settle this point, Professor Donders carefully investigated the accommodative power of patients who had been operated on for cataract.

His method of testing the presence or absence of this power was a very exact one, and depended on the usual presence of a slight degree of astigmatism (due to the form of the cornea) in the eyes examined, from which it followed, that in the middle of the focal range a point of light appeared as a circle, but by the slightest alteration of the refraction the circle would become converted into an ellipse. Thus the very slightest act of accommodation could be detected. His examinations of such patients invariably indicated the absence of all power of accommodation.

The cases published by Professor Förster, he maintains, do not demonstrate the presence of accommodative power, the tests applied being quite insufficient. The fact that in the cases reported by Förster the degree of sharpness of vision was not equal at different distances, is sufficient to condemn his theory.

At the request of Donders, Mr Coert examined a number of patients (from whom the lens had been removed) with respect to their power of vision at different distances. The patients were provided with a lens with which they saw most distinctly at 10 feet or 15 feet distance, and the sharpness of vision noted. The test-types were then brought nearer to the eye, when it was invariably found that the degree of acuity of vision (in proportion to distance) gradually diminished even in those cases where contraction of pupil and an increased convergence of the covered eye showed, beyond doubt, an endeavour to induce accommodation. Three cases, in the examination of which Professor Donders himself took part, are detailed.

A series of investigations was also made into the influence of "circles of diffusion" upon sharpness of vision. These investigations completely corroborated the view, that the so-called range of accommodation in aphakia was only apparent, not real.—*Archiv. für Ophthalmol.*, Band xix. Abth. i.

DR WOINOW, MOSCOW, ON THE POWER OF ACCOMMODATION IN APHAKIA.

Dr Woinow, while maintaining with Helmholtz, Donders, etc., that in the normal eye accommodation entirely depends upon, and may be explained by, an alteration of the curvature of the lens, still thought it possible, that in aphakia a slight power of accommodation *might* exist, attributable to the action of other organs, which in the normal eye took no part in the act of accommodation. He consequently carefully examined eleven patients on whom he had operated for cataract, and in whom the resulting vision was particularly good. His method of examination consisted in placing before the patient's eye a suitable convex lens, the posterior surface of which was blackened, with the exception of a spot, 3 mm. broad, to represent the pupil. The patient's head and the lens were fixed in a suitable apparatus to prevent the motion of either. In front of the patient's eye was placed a stand, to which was attached a shade, having a slit ½ mm. broad, over which was placed a double cobalt glass. By a graduated scale on the table, the distance between the shade and the patient's eye could be accurately measured. Behind the shade was placed a bright lamp, with a convex lens before it. The patient's head being fixed and the room darkened, the stand containing the shade is moved backwards and forwards, and the patient is desired to state the exact point at which the violet-coloured slip appears to have blue or red borders. The distance through which the shade may be moved without the appearance of coloured borders, indicates the range of accommodation. To test the delicacy of this method, Dr Woinow paralyzed the accommodation of one of his own eyes by means of atropine, and, employing this apparatus, found that the shifting the stand to the extent of 2 to 3 mm. sufficed to produce distinct coloured margins. The result of his investigation of the eleven cataract cases was, that an average range of accommodation of $\frac{1}{40}$ was abtained. He further found in one of these patients that the application of atropine reduced his accommodative power from $\frac{1}{48}$ to $\frac{1}{40}$.—*Archiv. für Ophthalmol.*, Band xix. Abth. iii.

ON THE ACCOMMODATION-MOVEMENTS OF THE CHOROID IN THE EYES OF MEN, APES, AND CATS.

Professors Heusen and Völckers of Kiel, in 1868, in a pamphlet they published (Ueber den Mechanismus der Accommodation), found, as a result of a series of experiments on dogs, that when the ciliary muscle was stimulated—among other effects produced—the choroid was stretched, its anterior extremity being drawn forwards. Their method of demonstrating this point was to remove a portion of the orbit, and lay bare the ciliary nerves and side of the eyeball. They then passed three needles into the interior of the eye, in the equatorial region. They also passed one through the ciliary muscle, and one through the region of the macula lutea. On stimulating the ciliary nerves, the needles passed through the ciliary muscle and the region of the macula lutea were unaffected, while the other three moved in a direction indicating a drawing forwards of the choroid. They got exactly similar results from experiments on an ape (*Cebus capucinus*). Similar results were obtained in experimenting on cats. As they further found that, by passing a stream of water of the temperature of the body upon an eye removed from a dog, the power of producing contraction of the ciliary muscle by stimulating the ciliary nerves remains for fifteen minutes, they were led to attempt similar experiments on freshly-enucleated human eyes. After several unsuccessful attempts (where the failure was evidently due to the diseased state of the ciliary muscle), they eventually succeeded, in the eye of a boy that was enucleated on account of staphyloma of the upper part of the cornea, in getting exactly similar movements to those they obtained in the eyes of the lower animals.—*Archiv für Ophthalm.*, Band xix. Abth. i.

ON THE EYE AFFECTIONS THAT ACCOMPANY LEPRA.

Dr Pedraglia of Madrid, while resident at Brazil, was struck with the large proportion of patients suffering from lepra who also laboured under certain eye affections, which presented a specific, peculiar character, whereby they differed from similar diseased conditions occurring in persons not affected with lepra. The eye symptoms generally found were as follows:—Loss of eyelashes without the previous occurrence of blepharitis ciliaris. Participation of the skin of the eyelids in the prevailing cuticular affection, whether it be thickening, tubercular formation, or atrophy. A watery condition of the eye, partly from increased lachrymal secretion, but chiefly from slight eversion of the lower lid, due to the conjunctiva also having become affected with leprous thickening (this occurs particularly when the skin is reddened and thickened). A form of superficial inflammation of the cornea leading to lasting opacities, conical projection, or atrophic shrinking. In almost all cases where corneal opacity is present, there are also symptoms of affection of the uveal tract, such as—degeneration of iris tissue, adhesions between the iris and cornea or lens, opacities of vitreous humour, incipient cataract, and tendency to general atrophy of the globe.

Out of 26 lepers in the Lazarus Hospital of Bahia on the 24th of April 1869, 8 presented the eye symptoms described. These cases, and 6 others that occurred in Dr Pedraglia's practice at Rio de Janeiro, are fully narrated.—*Klin. Monatsbl. für Augenheilk.*, 1872.

(*To be continued.*)

THE NATURAL HISTORY OF INFECTIOUS DISEASES.—1. Fluctuations in the period of incubation. The duration of incubation (for measles, variola, varicella, parotitis, rubeola, typhus, 2 weeks; typhoid, an average of 3 weeks; scarlatina, 5-7 days; syphilis, 10 weeks) shows a certain agreement according to weeks or fractions of weeks. External temperature can have an influence on the duration of this stage, as is shown by the early outbreak of certain exanthemata in summer, or on parts of the skin exposed to the local action of heat. Certain examples tend to show that, like medicines, the effect of certain infecting matters appears with varying rapidity, according to the part where they are received into the system; for example, through direct inoculation or inhalation. Certain other circumstances, external influences, as chills, faults of diet, menstruation (especially, adds the reporter in the *Centralblatt*, mental influences) can shorten the duration of incubation. The same thing probably holds good for medicaments; while, according to Gerhardt, it is very improbable that the quantity of the infecting material taken up has any influence in regard to the duration of the period of latency. On the other hand, in this relation the stage of development in which the infecting matter is at the time of its reception seems of importance.

2. As pelioma typhus, Speyer (1850) was the first to describe diffuse bluish-red spots which show themselves in the course of typhus, and which since that time have been observed by several authors in the severer and lighter forms of typhus on different parts of the body. Gerhardt describes a very well-marked case.

3. Relapses of acute infectious diseases must be distinguished from second attacks. The first are due to two distinct infecting processes occurring the one shortly after the other, the latter to the insufficient protecting power of a first attack. True relapses occur in a period of time which, reckoning from the first outbreak, does not essentially go beyond the period of incubation and sickness, and so comport themselves as if a new infection had followed during the still existing disease, and as if the latter had not conferred complete immunity. They come in all infectious diseases, are mild, and of short duration.

4. Parotitis epidemica is reckoned by Gerhardt amongst infectious diseases, because, as has been shown by frequently repeated measurement, there is never complete absence of fever temperature, and there is constantly considerable swelling of the spleen and many of the lymphatic glands. The so-called metastases to the sexual organs are to be understood as secondary localizations. Further, the function of the parotid gland is not checked. The secretion flows indeed, as Gerhardt found in one case, somewhat slower than usual, but is clear, and of undiminished saccharifying power. According to the seat of the affection, it might be named periparotitis.—Extracted in *Centralblatt*, No. 2, from the *Deutsches Archiv f. Klin. Med.*, 1873, XII., 1–12.

ON THE ETIOLOGY OF VARIOLA. *Preliminary communication by Dr W. Zuelzer, Berlin.*—Zuelzer cites several of a series of experiments into the nature of the infection.

1. *Feeding experiments.*—A quantity of about 3–4 grammes of blood from a case of fatal variola, and the same quantity of pus from the maturing pustules of a case of variola of the third grade, were rubbed up with bread-crumbs into small balls. With these two small monkeys (spec. *Cercopithecus*) were fed. Both animals remained healthy.

2. *Experiment by trying to infect through the uninjured skin.*—Ten days later, the hair was cut from a portion of the skin of the back with scissors, without injuring the cutis. The part was rubbed gently with charpie that had been soaked with variola-pus. The charpie was then covered with a watch-glass, which was fastened on the skin by sticking-plaster. After three hours, the charpie was removed and the part washed. Here also no infection followed.

3. *Inoculation.*—The same monkey was twelve days later inoculated in several places on the back and inner surface of one thigh, with fresh blood from a case of rapidly lethal variola. On the sixth

day after the inoculation, the rectal temperature of the monkey, which had hitherto been normally 100·2° to 101° in the morning and 101° to 102·5° in the evening, began rapidly to rise, and remained feverish until the eleventh day (105·4° to 106·5°). The creature lost its appetite, which, however, on the ninth day had already returned. Red spots, which rapidly developed to papules, some of which became flattened quickly-bursting pustules, appeared in greatest number on the tail, and discretely on the back, the inner surface of the thighs, and the pharyngeal mucous membrane.

4. *Infection through inspiration.*—To produce infection, a large quantity of the severed crusts and small pieces of linen that were impregnated with blood and pus from different variola patients, were used. Several small gauze bags were filled with these masses and were put into the wooden cage, which was provided with wire-work only on one side, and frequently strongly shaken. A small wire basket filled with similar masses was given the monkey to play with. The animal sickened on the fifteenth day under the same appearances as the other monkey.

The result of these experiments as regards the etiology of variola is as follows:—

1. The blood of variola patients is infectious.
2. Infection does not take place through the medium of the digestive apparatus, and also probably not through the uninjured skin.
3. The disease can be conveyed through the inspired air when this is sufficiently impregnated with the poison of the disease as well as by inoculation.—*Centralblatt*, No. 6.

CASE OF EXTIRPATION OF THE KIDNEY.—Dr Brandt, Professor of Surgery at Klausenburg, relates in the *Wiener Med. Woch.* for 29th Nov. and 6th Dec. an interesting case of the successful extirpation of a kidney after an accidental injury.

S. P., a healthy peasant, aged 25, was, on 3d June 1872, stabbed by a drunken man in the left hypochondrium by means of a pointed table-knife. Three or four ounces of blood were lost; and after a while attacks of cough came on, which were attended by a protrusion of a fleshy swelling through the narrow orifice of the wound. Twenty-four hours afterwards he was brought to the hospital, when scarcely any signs of anæmia were observable. Exactly opposite the lower edge of the last false rib there was found a flattish fleshy tumour, of a reddish colour, measuring nine centimetres in length and six in breadth, and from which constantly trickled a transparent, straw-coloured fluid. The edges of the tumour were irregular, as if gnawn, and its under surface was covered by a smooth membrane, in part of a dark-red and in part of a whitish colour. From the middle of this lower surface, a pedicle, six centimetres in circumference, extended into the wound of the abdomen, giving the tumour the appearance of a mushroom.

This pedicle was surrounded also by a smooth membrane, which was torn in several places, whence escaped some of the abovementioned fluid. It was firm to the touch, evidently containing vessels, which, however, conveyed no pulsation. The oblique wound in the hypochondrium through which this pedicle passed was about three centimetres in length and one and a half in breadth, having sharp edges, with which it embraced the pedicle. To the touch the tumour was moderately firm and painless (pain only being produced by traction on the pedicle), and its temperature at the surface was lower than that of the rest of the body. The tumour upon the whole much resembled the section made on examining a kidney, and under the microscope the shades of colour characterizing the cortical and pyramidal substances were made out. The abdomen in the vicinity of the wound was quite normal in appearance, but in the left lumbar region there was a rema kable oval-shaped sinking-in. The tumour was very movable on its pedicle when rotated, and this last admitted of some elongation, which, however, produced great pain in the pedicle and the abdomen, but not in the tumour itself. The general condition of the patient was unexpectedly good, the pulse being, twenty-four hours after the accident, about 80, and the temperature 37·8° C.

The appearance of the tumour and the analysis of the urine which trickled away (which is given in some detail) showed plainly that it was the kidney that had suffered an injury. The organ had evidently been cloven to within its calyces; but what amount of injury the ureter and the vessels had undergone could not be ascertained. It could hardly have been supposed that a knife could have been driven through the abdomen right into the kidney without injury being done to the peritoneum; but the absence of all symptoms proved that such had been the case. After considering at some length the various probabilities of the course the case would take if the kidney were allowed to remain where it was, and of the effects which its removal might exercise on the economy, Professor Brandt proceeded to its extirpation four days after the injury. The patient, indeed, continued in a very favourable state for the operation, having the day before its performance walked some distance in order to be photographed, occupying two hours in assuming the necessary positions. For the three days prior he had passed little urine, and this contained albumen, derived at all events in part from the injured kidney, which at its injured surface furnished it abundantly. On the day of the operation the pulse was 70, and the temperature 37·6°. The purulent secretion from the injured kidney had considerably increased. A double silken ligature was passed by means of a straight needle through the middle of the pedicle, which was tied in two portions, the kidney itself being separated by means of the knife. No hæmorrhage followed. A daily report of the condition of the patient and the quantity of urine secreted is given from 7th to 22d June. From this it results that considerably more than half of the urine usually secreted by the two kidneys was furnished at all times by the single one, and on some occasions this amounted to the entire normal quantity. In the whole course of the case no symptoms of an uræmic or peritonitic character occurred. The patient has been frequently seen since his discharge, and continues well.

SUDDEN DEATH FROM EMBOLISM OF THE PULMONARY ARTERY, AFTER INFLAMMATION OF VARICOSE VEINS.—M. Chabenat in an inaugural thesis gives the following conclusions on this important subject:—

1. Inflammation of varicose veins of the adhesive type predisposes to embolism of the pulmonary artery, and, as a consequence, to sudden death.
2. The clot breaks up and is carried on by the current of blood when the patient is in process of cure, and generally as the result of an effort.
3. Death is due in general to the size of the clot, which completely blocks up the pulmonary artery or one of its large branches.
4. The patients die almost always without asphyxia, rarely by fainting; sometimes asphyxia is preceded by a faint.
5. As yet we have no means by which to prevent the migration of the clots; it is necessary then, as far as possible, to prevent them from breaking up.—*Gazette Médicale de Paris*, No. 11, 1874.

A NEW SIGN OF DEATH.—M. E. Bouchut, already known to science by his researches on this subject (for in 1849 he showed that the formal sign of death consisted in the cessation of the beats of the heart, which can be made out by auscultation, and, if necessary, by the use of an acupuncture needle), has lately published a note on a new sign of death—a pneumatosis of the retinal veins. This is easily appreciable by the ophthalmoscope, and, M. Bouchut says, is an immediate and a certain sign of death. The veins show an interruption here and there to the current of blood within them, like that seen in the column of a thermometer composed of coloured alcohol.—*Gazette des Hôpitaux*, No. 29, 1874.

CONTRA-INDICATIONS OF OPERATIVE INTERFERENCE IN CASES OF MELANOTIC TUMOUR DERIVED FROM A MICROSCOPIC EXAMINATION OF THE BLOOD AND OF THE URINE.—The state of the viscera is of the utmost importance to a surgeon proposing to operate in a case of melanotic tumour. Is it possible to learn anything of this before operating? In patients suffering from visceral melanotic disease, the blood and the urine have the following characters:—The white corpuscles of the blood are increased in numbers, and the serum contains numerous brownish red granules and flexible granular tubes, which appear to be moulds of capillaries; and the red globules, in mass, present a more or less colour of sepia. The urine is dark in colour, and, on the addition of nitric acid and bichromate of potash, becomes blackish; and contains cylindrical masses, and collections of brown granules. This melanuria does not necessarily indicate metastatic nodules in the kidney, but implies an excess pigment in the blood; and, as a consequence, the spread of disease in the viscera. (The above generalizations, so far confirmed by cases, are quoted in brief abstract from an interesting note communicated by M. Nepveu to the Society of Biologie, on 17th January 1874.)—*Gazette Médicale de Paris*, No. 5, 1874.

Part Fifth.

MEDICAL NEWS.

THE following account by Sir William Fergusson of his examination of the body of Livingstone will be read with much interest:—

"It has fallen to my lot to have the honour of being selected to make the crucial examination to this end, and I have accordingly performed that duty. From what I have seen, I am much impressed with the ingenious manner in which those who have contrived to secure that the body should be carried through the long distance from where Livingstone died until it could reach a place where transit was comparatively easy, accomplished their task. The lower limbs were so severed from the trunk that the length of the bulk of package was reduced to a little over four feet. The soft tissues seem to have been removed to a great extent from the bones, and these latter were so disposed that by doubling and otherwise the shortening was accomplished. The abdominal viscera were absent, and so were those of the chest, including, of course, the heart and lungs. There had been made a large opening in front of the abdomen, and through that the native operators had ingeniously contrived to remove the contents of the chest as well as of the abdomen. The skin over the chest, sternum, and ribs had been untouched.

"Before these points were clearly ascertained, some coarse tapes had to be loosened, which set free some rough linen material—a striped coloured bit of cotton cloth, such as might have been an attractive material for the natives among whom Livingstone travelled—a coarse cotton shirt, which doubtless belonged to the traveller's scanty wardrobe, and in particular a large portion of the bark of a tree, which had formed the principal part of the package—the case thereof, no doubt. The skin of the trunk, from the pelvis to the crown of the head, had been untouched. Everywhere was that shrivelling which might have been expected after salting, baking in the sun, and eleven months of time. The features of the face could not be recognised. The hair on the scalp was plentiful, and much longer than he wore it when last in England. A moustache could not be distinguished, but whiskers were in abundance. The forehead was in shape such as we are familiar with from memory, and from the pictures and busts now extant. The circumference of the cranium, from the occiput to the brow, was 23⅜ inches, which was recognised by some present to be in accordance with such measurements when alive.

"In particular, the arms attracted attention. They lay as if placed in ordinary fashion, each down by the side. The skin and tissues under were on each side shrunken almost to skeleton bulk, and at a glance to practised eyes—there were five, I may say six, professional men present—the state of the left arm was such as to convince every one present who had examined it during life, that the limb was Livingstone's. Exactly in the region of the attachment of the deltoid to the humerus, there were the indications of an oblique fracture. On moving the arm, there were the indications of the ununited fracture. A closer investigation and dissection displayed the false joint which had long ago been well recognised by those who had examined the arm in former days. The Rev. Dr Moffat, and in particular Dr Kirk, late of Zanzibar, and Dr Loudon, of Hamilton in Scotland, at once recognised the condition. Having myself been consulted regarding the state of the limb when Livingstone was last in London, I was convinced that the remains of the great traveller lay before us. Thousands of heads, with a like large circumference, might have been under similar scrutiny; the skeletons of hundreds of thousands might have been so; the humerus in each might have been perfect; if one or both had been broken during life, it would have united again in such a manner that a tyro could easily have detected the peculiarity. The condition of ununited fracture in this locality is exceedingly rare. I say this from my personal professional experience; and that such a specimen should have turned up in London from the centre of Africa, excepting in the body of Dr Livingstone, where it was known by competent authorities to have existed, is beyond human credibility. It must not be supposed by those who are not professionally acquainted with this kind of lesion—which often causes so much interest to the practical surgeon—that a fracture and new joint of the kind now referred to, could have been of recent date or made for a purpose. There were in reality all the indications which the experienced pathologist recognises as infallible, such as the attenuated condition of the two great fragments (common under such circumstances), and the semblance of a new joint, but actually there was a small fragment detached from the others which bore out Livingstone's own view, that the bones had been 'crunched into splinters.' Having had ample opportunity of examining the arm during life, and conversing with Livingstone on the subject, and being one of those who entertained hopes that the last reports of Livingstone's death might, like others, prove false, I approached the examination with an anxious feeling regarding this great and most peculiar crucial test. The first glance at the left arm set my mind at rest, and that, with the further examination, made me as positive as to the identity of these remains as that there has been among us in modern times one of the greatest men of the human race—David Livingstone."—*British Medical Journal.*

In the *New York Medical Journal* for December 1873, there is an interesting paper, by Dr R. W. Taylor, on four cases of ulcer of the penis, occurring in infants after circumcision. The interest of the paper centres in this—that the cases were all operated on by the same person, and occurred within three months of each other. The object of Dr Taylor's investigations, undertaken at the request of the Board of Health, was to ascertain whether the operator had been the means of infecting the children with syphilis, of which he had been accused. The result was, that the accusation fell to the ground; for it was found that three of the cases were doubtful syphilis, and no trace of syphilis could be discovered in the operator. It is matter for regret that Dr Taylor was unable to discover the cause of infection in the only case which was unmistakable syphilis. The difficulties of the investigation are described by Dr Taylor himself thus:—"The investigation of these cases has been attended with several drawbacks, and under somewhat unfavourable circumstances, having been entered upon nearly a year after the commencement of the trouble, after two children had already died, among a low and ignorant class of people; and I being unable in several instances to obtain desired information on important points, it is utterly impossible to present all the particulars of the cases."—(P. 563.)

The practical result, however, of this investigation is a recommendation to the Board of Health, under the following heads:—

"1. That in the Jewish rite of circumcision there is a possibility of the occurrence of syphilis.

"2. That the contagion is most likely to be communicated in the act of sucking the wound, the mouth containing a styptic liquid; and that, perhaps, it may occur by means of instruments soiled by syphilitic blood.

"3. That the chances of such contagion are rendered greater by the performance of the operation by irresponsible, non-professional persons.

"4. That the operation of sucking should be wholly abolished; and that, if a styptic solution of any kind be used, it should be poured from a vessel on the wound rather than squirted upon it from the mouth of the operator.

"5. That in no instance should two or more children be thus operated on consecutively, without a thorough cleansing of the instruments and utensils used after each operation; and that, in every instance, the greatest care should be taken in cleansing the instruments.

"6. That the performance of the rite should be absolutely confined to responsible and educated persons; either a physician alone [why not a surgeon?] being selected, or a physician assisting an officiating rabbi, or a circumciser of recognised merit.

"7. That, under these circumstances, accidents of any kind are reduced to a minimum."

One of the most interesting facts brought out incidentally by Dr Taylor's investigations is, that this apparently severe operation of circumcision is seldom followed by grave accidents in infants.

Death of M. Muron.—This talented and most diligent young surgeon, one of the sub-editors of the *Gazette Médicale de Paris*, died lately under very distressing circumstances, on his return from London, where he had been zealously attending hospital work, and where he had made most copious notes, and obtained much information. His illness, which lasted only one week, began by a small abscess of the nostril; this was opened, but it gradually spread into an extensive carbuncle, and eventually involved forehead and eyes in general swelling, ending in furious delirium, and death from cerebral meningitis.

Death of Joulin of Paris.—This remarkable versatile genius had also vast power of work, and, from being a shopboy, workman, journalist, caricaturist, and medical student, he, by the sheer force of intellect, founded a Journal of Obstetrics, to which he gave his name, wrote many obstetrical works, became a professor *agregé*, and made a practice. He was a well-known contributor to *Figaro*, and possessed a trenchant yet amusing style. He died of a rapid attack of cerebral hæmorrhage.

CORRESPONDENCE.

LARGE DOSES OF THE BROMIDE OF POTASSIUM IN EPILEPSY.

To the Editor of the Edinburgh Medical Journal.

Sir,—In the "Periscope" for this month is a letter from Dr Begbie to Dr Anstie on the "Bromide Question." Though I endorse much, I do not agree with all contained in the above communication anent the behaviour of epilepsy when treated by the bromide of potassium. Unfortunately, the number of cases treated as below are numerically small, but they were under daily supervision; further, I cite from a not over-retentive memory, which may seem somewhat to invalidate the conclusions. In spite of these drawbacks, I am nevertheless convinced that epilepsy may not only be modified, or even warded off, but absolutely abolished, whilst under the sufficient action of the bromide. There is one error into which every practitioner is apt to fall—a cessation of convulsions as the result of the drug. This error can be eliminated by suddenly stopping the medicine.

Should the paroxysms not return, this lull may be directly attributed to the *vis medicatrix naturæ;* conversely, then, the administration of the bromide has temporarily held them in abeyance. Wait for a week or two, and the malady will gradually declare itself. Possibly, were the drug continued for a lengthened period, the system might be so gravely affected that every trace of epilepsy might vanish.

The dose of the bromide fluctuates from five to fifteen grains. The more cautious would employ the former; the bolder practitioner, the latter amount. *Mediis tutissimus ibis.* Let the golden mean be employed of ten grains four times a day, or rather every six hours, to an epileptic having, say, six seizures diurnally. After a week, and whilst taking forty grains daily, the convulsive attacks occur four times in twenty-four hours, increase the dose gradually to fifty, sixty, seventy grains, and onwards daily. With an increasing ratio of the dose, there is, inversely, a decreasing ratio of the seizures, the decrement progressing from four to three, two, one, until they eventually disappear. I believe that as much as seventy, or even seventy-five grains, have been administered for a single dose. Two hundred and forty grains in the day I have not unfrequently employed.

In the use of this drug in gradually increasing quantity, there are certain indications urgently calling for an immediate cessation of the remedy. These are, an overpowering somnolence, thickness of articulation, and deficient locomotive co-ordination, thus presenting a striking resemblance to the last stage of intoxication. When all or any one of these in the slightest degree obtains, the drug requires to be at once omitted until they disappear. When this takes place, the anti-convulsive may be exhibited in somewhat lesser doses. The therapeutic amount, wherein the paroxysms of the disease cease, yet the toxicological symptoms of the drug do not appear, varies with different individuals. With some, eighty grains suffice; with others, a hundred; with others, again, considerably more. The line of demarcation between the therapeutic and toxicological effect can only be discovered empirically; but, once discovered, becomes a boon to the patient and a source of gratulation to the physician.

WILLIAM MACFARLANE, M.D.

18 Argyll Road, Kensington,
London, 30th March 1874.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Clinical Lectures on Diseases of the Heart.* By GEORGE W. BALFOUR, M.D., F.R.C.P.E., Physician to the Royal Infirmary, Edinburgh.

On the Diagnosis of Cardiac Disease generally, with reference to the Symptoms and the Physical Signs.

GENTLEMEN,—In gathering together the prominent facts in relation to the circulatory system in any case, few things are more striking than the frequently apparent irrelevance of the symptoms in those who are otherwise ascertained to be examples of serious cardiac disease, if we perhaps except the equally remarkable fact, that cardiac symptoms are frequently complained of when no cardiac disease exists. Thus, should a patient come to you complaining of palpitation, irregular action, or of his heart generally, you may at once assure him, without much fear of being wrong, that his heart is all right, that he is only weak, nervous, and probably dyspeptic. Almost the only exceptions to this rule are old hospital patients, who, from having been so often lectured over, are almost as well acquainted with their own special lesion as an average practitioner.

Of course, if pain be a predominant symptom, that is usually referred to the region of the heart, but not always to the organ itself; and we continually meet with cases in which serious valvular lesion has existed for many years altogether unnoticed by the patient, even although he may have been leading an active and laborious life. Such a lesion is said to be mute; that is, it has been perfectly compensated, and it never asserts itself as a disease until that compensation has been ruptured by accident, or by the gradual advance of those organic changes which are inseparable from it. In the one case the ruptured compensation is reparable, and comparative health may be restored; in the other, it is irreparable, though judicious treatment may prolong life and postpone the inevitable end.

A truly cardiac patient—one suffering from actual disease of the heart—as a rule comes to you complaining, not of that organ, but of one or other of the secondary results of his lesion. He complains of breathlessness or of dropsy, either or both of which may result from that lesion if it be uncompensated, or if the compensation be ruptured.

If the patient complain of shortness of breath, as is often the case, you will find that this cardiac breathlessness presents certain distinctive features wherein it differs from pulmonary breathlessness, the most striking of these being the perfect tranquillity of the breathing while the patient is at rest, at the same time that any exertion at once produces so anxious a desire for more air as can be expressed by no fitter term than the *air-hunger* of the Germans. The amount of lesion is not to be measured by this breathlessness, but its seriousness, as dependent upon the degree in which the compensation is ruptured, may certainly be so. The patient may only puff considerably in going up a hill or ascending a stair, or his shortness of breath may be so great as speedily to compel him to call a halt on attempting either of these feats; or it may be so extreme as to prove distressing on making such perfectly trifling exertions as merely sitting up or turning in bed. At the same time there is no true dyspnœa, or difficult breathing properly so called; there is no obstruction either to inspiration or expiration; there may even be no curtailment of the air-space in the lungs from any cause whatever; the breathing while the patient is at rest is perfectly quiet and natural: yet such is the difficulty—from cardiac causes—of getting the blood aerated, that the slightest exertion produces such a gasping inquietude as is extremely characteristic. This is one form of cardiac asthma, as it is termed; now and then we have another, in which the breathlessness, though not dependent upon exertion, is yet equally independent of pulmonary lesion. In this case the patient wakes gasping and alarmed from his first sleep; he has palpitation, occasionally pain (angina), almost always irregular action of the heart, which is always feeble; now and then the patient is sick, and sometimes vomits a mouthful or two. This form of cardiac asthma is mostly senile in character, and associated with muscular degeneration rather than with valvular lesion. It frequently arises from some slight gastric derangement, which reflexly affects the enfeebled heart in an injurious manner; and it is often the beginning of the end to those affected—the first intimation that the "pitcher is broken at the fountain," and that death has already seized the very citadel of life.

Such patients, however, never come to you—you are always sent for to see them; and I have only mentioned this affection now to illustrate the fact, that exertion is not always necessary to produce cardiac breathlessness, and that even in this case the panting is characteristic, while the absence of pulmonary lesion marks its cardiac origin.

To produce so-called cardiac breathlessness, however, it is not necessary to have actual cardiac disease. Breathlessness depends upon imperfect aeration of the blood, and in the absence of pulmonary lesion may depend upon lesion of the heart or of the blood itself. Even though a patient, then, presents all the characteristic symptoms of cardiac asthma, we must not therefore set him down as certainly labouring under cardiac disease: he may be only anæmic. But inasmuch as anæmia and cardiac disease frequently coexist, the assured presence of the former, evinced by the bloodless condition of the lips, gums, etc., does not exclude the latter. The presence of a breathlessness having the characteristic symptoms described, makes us certain that we have to do with a hæmic or a cardiac lesion; which it is, we must determine by further inquiries.

Dropsy depending upon cardiac disease always[1] begins across the instep, and gradually fills up the lower extremities, the face and upper parts of the body remaining free. But inasmuch as the œdema of simple debility commences in the same position and pursues the same course, such a dropsy cannot be accepted as certainly indicative of the existence of cardiac disease, but must only be received as a hint that possibly the heart may be affected.

When a patient, then, who breathes easily when at rest complains of breathlessness on exertion, or of swelling of his feet, with or even without marked breathlessness, we suspect the heart to be the organ at fault, and we proceed to confirm or to set aside that suspicion by further investigation.

In further examining the condition of our patient, we first feel both radial pulses simultaneously, noting whether the arteries are firmer or more tortuous than usual (atheroma). If there be a marked difference between the two radial arteries, we feel both brachials simultaneously; if these be equal, the difference between the two radials is due to irregular distribution. If the brachials differ, in all probability there is some abnormal physical cause to account for it—possibly an aneurism, the mode of detecting which we shall afterwards describe. Should the radial pulses be equal and regular, but small and feeble, we elevate the wrist to a level with the head, if the patient be standing or sitting; if lying, we elevate the arm to its full length perpendicularly to the body. Should the pulse then become extinguished, or nearly so, the patient is anæmic, and possibly anæmia is his sole disease; but we must never under any circumstances rely upon one symptom, however apparently trustworthy, but merely note it as an aid and a guide in our further investigation. Should the pulse, after elevation of the arm, remain still small and feeble, but distinct, the cardiac disease, if present, is mitral. Irregularity of the pulse confirms this suspicion; extreme irregularity points to the probability of the affection of the mitral valve being constriction rather than dilatation. Should the small, feeble, and possibly irregular

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[1] "Almost invariably," says Walshe, any exception being excessively rare. Diseases of the Heart, p. 302: 1862.

Alterations in the position and extent of the apex beat are also readily appreciable by the eye. These must be noted accordingly, to be afterwards more fully investigated by palpation. Pulsation is also frequently seen in the epigastric region. Sometimes this is associated with absence of the apex beat from its usual position, and is to be regarded as one form of its displacement—a displacement which may be brought about in various ways, the commonest of these being dilatation of the right ventricle, by which the left ventricle is pushed backwards, the right one communicating its impulse to the lower part of the sternum and to the liver, which is then seen to pulsate in the scrobiculus cordis. It may be doubted if such pulsations are ever visible in a perfectly normal condition of the heart and neighbouring organs. Assuredly, they are often seen where no actual cardiac disease exists; simple dilatation of the right ventricle is invariably more or less present when pulmonary congestion exists, even from such simple and temporary causes as strenuous exertion or bronchial catarrh; and whenever dilatation of the right side exists to any considerable extent, epigastric pulsation may be seen. It is rendered more perceptible by any cause which may effectively conduce to the transmission of such impulse to the abdominal walls. Thus it is sometimes favoured by the existence of effusion in the pericardium, but especially by the occurrence of enlargement of the liver, which is so frequent a concomitant of dilatation of the right ventricle. Indeed, in such circumstances, the systolic impulse of the venous regurgitation itself is occasionally so great as to induce actual pulsation of the whole liver, a pulsation which is then visible, not merely in the scrobiculus cordis, but throughout the whole right hypochondriac region;[1] an extent of hepatic pulsation which is not, however, always distinctive of great venous regurgitation, as it is occasionally seen as the result of the impulse communicated by a large aneurism lying immediately above the liver.[2] There are other pulsations in

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[1] This form of hepatic pulsation was first described by Allan Burns, in his Observations on some of the most Frequent and Important Diseases of the Heart, Edin. 1833. At p. 265, he quotes a case from Senac, in which direct pulsation was communicated to the epigastrium by a vena cava inferior, the size of a man's arm. At p. 266, he also mentions a case in which he states that epigastric pulsation was produced by repercussion from solidified lungs.

[2] Extract of a letter from a late physician labouring under aneurism of the thoracic aorta projecting through the sternum:—"I am satisfied that your view of the origin of the liver pulsations from the aneurism mainly is the correct one, however discouraging to myself that may be. But I have this to say, on the more cheerful side, that if an *enlarged* liver has had nothing to do with them (as Dr —— says), and which by decreasing in bulk has caused them to be less felt, the very great decrease in them which has occurred since I began the iodide, is very favourable to the conclusion, that under its action the sac must have contracted considerably. Early in February, before I had reason to suspect any increase of the liver, the pulsations had the effect of expanding the opposite sides of the hypochondria at each beat, an effect which is not now perceptible, though I am supposing the liver to be larger, in which, however, I may be mistaken." Dated 3d May 1870.

the epigastric region not depending upon cardiac impulse, such as the ordinary pulsation of the abdominal aorta, which occasionally becomes visible through the great emaciation of those previously of full habit, or which may become visible by their more energetic transmission to the abdominal surface by some overlying tumour. Now and then this pulsation is actually aneurismal in character: much more frequently it is simply neurotic, and yet limited to the abdominal aorta. Under some of these circumstances we can readily understand that the pulsation extends downwards along the course of the aorta; under others, as when propagated upwards by a solid tumour, it is obvious that it may not only be limited in its longitudinal propagation, but may even extend transversely. Inspection directs the attention to these pulsations indicating to an experienced eye their probable nature, which can only be accurately determined by other means of exploration. In rare instances, a pulsatory movement is also communicated to the epigastrium through the movement of the heart's apex during the ventricular systole, pulling upwards an adherent pericardium, diaphragm, and liver.[1] This movement is, of course, exactly the reverse of that in ordinary epigastric pulsation. As this extensive adhesion is, as a rule, only the result of a severe and extended inflammation, affecting uniformly the whole surface of the heart, we can readily understand how, in such circumstances, a universal undulatory movement may be perceived, in which, when the heart's action is at all rapid (over 90 beats per minute), it may be difficult to say what parts of the motion are systolic and what are diastolic. In these circumstances, the variations in time between the movements of any two parts are readily rendered visible by attaching to each, by means of a pellet of beeswax, a bristle carrying a small paper flag; and when the pulse is over 90, this is the only way in which such differences can be ascertained with any certainty; and it is a means of attaining certainty of diagnosis often of much importance, especially when we have pulsations visible above the fourth rib, which may possibly be either aneurisms of the aorta or pulsations of the auricle. A comparison of the movements of a flag on the doubtful point with one over the apex, will settle the matter at once, as the beat of the auricles always precedes that of the ventricle, while, however near the heart an aortic aneurism may be, its pulsations can never precede those of the ventricles, but must succeed them by an interval which is more or less appreciable. The determination of this fact, we shall afterwards see, may be a matter of some importance in the diagnosis of cardiac lesions.

PALPATION confirms the information obtained by inspection, and adds somewhat to it. On placing the hand over the cardiac area, in many people with thin chest walls, we can distinctly perceive the alternate movements of auricular and ventricular systole, with each corresponding diastole; and, of course, we can also readily appreciate any pathological change in their movements. Great increase in their force indicates hypertrophy; but a diminution of that force is by no means to be regarded as a certain indication of atrophy, dilatation without hypertrophy, or even of cardiac debility from any cause,—though it may be a sign of one or other of these phenomena, as well as of great pericardiac effusion, pulmonary emphysema, or even simple thickening of the thoracic walls. In many of these cases, especially in pericardiac effusion, but more or less in all, the cardiac shock may be rendered perceptible to the hand, by causing the patient to sit up and lean well forwards. In this case, the readiness with which it can be appreciated, and the position in which it is felt, must be carefully noted. Forcible pulsation above the fourth rib, and within the cardiac area, may possibly be aneurismal, but most commonly depends upon dilatation and hypertrophy of the appendix of the left auricle. Forcible pulsation chiefly to the left of the cardiac area below the fourth rib, with depression of the apex beat, indicates dilatation and hypertrophy of the left ventricle; while pulsation beneath the lower part of the sternum, with disappearance of the apex beat, reveals dilatation with or without hypertrophy of the right ventricle, the extent of dilatation being to some extent measurable by the amount of epigastric pulsation, the degree of hypertrophy being denoted by the force of the pulsation. When the whole heart is hypertrophied and dilated, a more or less violent shock may be felt over the whole of the cardiac area; and where the hypertrophy is great, the shock may be double—the first forcible and systolic, the second less forcible and diastolic,—the result of the rebound of the enlarged heart from the posterior thoracic walls.

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[1] A remarkable instance of this will be found detailed in a note on p. 214 of Copland's Dictionary of Practical Medicine, vol. ii.

Displacement of the apex beat may be produced in the normal state by inclining the patient to the left side, when it may pass to the left a little farther even than a point equidistant between its normal position and the axillary line; and by inclining the patient to the right, the apex beat becomes faint or disappears, the right side of the heart being then felt to give a distinct pulsation in the epigastrium. In pericardiac effusion, the apex beat may be displaced upwards as far as the fourth interspace, and slightly outwards to the left. In pleural effusion on the left side, it may be turned quite round the other way into a similar position on the right side, and pleuritic effusion on the right side may displace it towards the left. In hypertrophy, with dilatation of the left ventricle, the apex beat passes downwards and to the left. In a similar condition of the right ventricle, it may entirely disappear from its normal position, a diffuse impulse at the lower end of the sternum revealing the cause.

When the left lung is retracted from the base of the heart from any cause, the pulsation of the pulmonary artery between the second and third ribs, close to the sternum, may be distinctly felt, and even the click of the semilunar valves perceived, because sounds consist of vibrations sufficiently rapid to become appreciated by the ear, and can always be both felt and seen if the propagating medium be suitable. Hence friction, the result of the rubbing together of the two layers of the pericardium when roughened by lymph, is easily perceived by the hand, and even the vibration of the valvular murmurs are readily enough appreciated. When rough, these vibrations have received the name of the purring thrill—*frémissement cataire*—because the sensation felt resembles that perceived on placing the hand on the body of a cat in the act of purring. Abnormal pulsations along the course of the thoracic aorta are usually confined to the ascending and transverse portions, and may be the result of simple displacement of the vessel, as occasionally happens in rickety chests, or they may be the result of aneurismal dilatations, and must be sought for in the tracheal fossa, as well as between the ribs along the course of the aorta, especially during expiration, when such pulsations, if faint, are most readily felt.

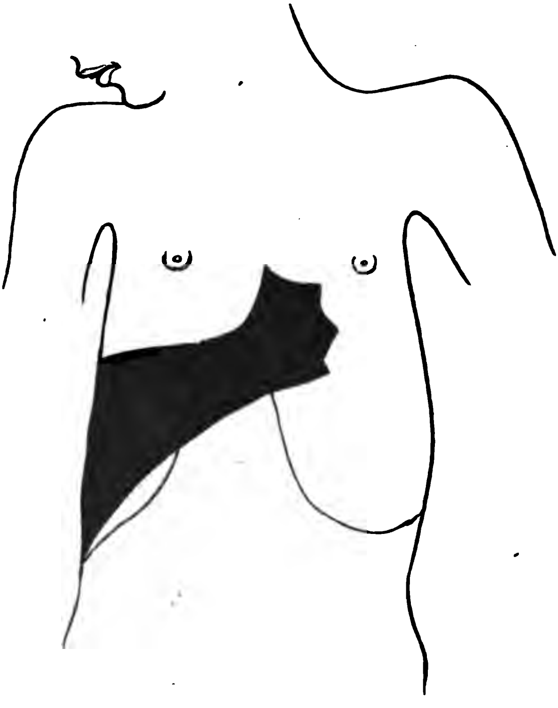
From all this we see that a very great amount of information may be obtained from palpation, the whole diagnosis frequently depending to a great extent on the information thus received. The points to be specially noted are—1*st*, The position of any perceptible pulsation; 2*d*, Its extent and force; 3*d*, Its rhythm, whether systolic, diastolic, intermittent, or irregular; and, 4*th*, Whether any vibratile sensations are perceptible over the seat of pulsation, and if so, what is their apparent rhythm.

Having thus registered all the information to be obtained from the inspection and palpation of our patient, we now proceed to PERCUSS him, and from this we procure information of a totally different character from any hitherto obtained. Up to this time we have been dealing with phenomena which can be seen and felt, but by percussion we can map out the entire cardiac organ, or rather we can accurately limit the extent of the cardiac dulness, even in those parts where movement is unseen and unfelt; and, as we shall by-and-by see, we thus possess ourselves of a most important link in the chain of facts which connects those visible and tangible phenomena with that hidden cause, the condition of which it is our object to elicit. Percussion of the heart is usually reckoned rather a difficult and unsatisfactory procedure; in reality, if we set about it in a right way, and correctly interpret its indications of the condition of the heart, it is perhaps one of the easiest problems which arise in the varied applications of this method of diagnosis. All that we can learn from percussion is simply the size, form, and position of the precordial dulness, but we can learn these accurately, and, as already said, these are facts which may materially aid us in our inquiry.

Percussion, in medical parlance, is the art of ascertaining the condition of the internal organs of the body, as to resonance, by percussing or tapping upon the surface of the body just exterior to the organ whose state we wish to examine. It is obvious that this condition may be modified, first, by the amount of air contained beneath the part percussed—the size of the resonant cavity; and, second, by the physical condition as to tension and structure of the parts on which we operate; and, in the living body, all of these circumstances may be variously modified. The heart itself, and the large bloodvessels with which it is immediately connected, contain, in the normal state, no air, and are never sufficiently tense to be themselves thrown into sonorous vibrations by percussion; when struck, therefore, mediately or immediately, they give forth no sound at all, or are simply said to be dull on percussion. The heart and large vessels are, however, in their natural positions, surrounded on three sides by the lungs, which are normally filled with air, and are therefore resonant; while, on the fourth side, the heart rests upon the liver, like itself, a dull, non-resonant body, from which we can, however, separate it more or less perfectly by measures presently to be described.

On percussing over the left side of the thorax, the veriest tyro can at once distinguish a more or less triangular area of complete dulness (Fig. 1[1]), which varies in size and shape in each individual, and indicates the size of that portion of the heart in direct contact with the thoracic walls; the magnitude of this is often

FIG. 1.



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[1] This figure is a diagramatic representation of the cardiac and hepatic ness of Fig. 4—organs healthy—of Sibson's Illustration of the Morbid

taken as an indication of the actual size of the heart itself, but it obviously only betokens the comparative degree to which the heart is uncovered by the lung, and as that may depend either upon increase of size of the heart itself or diminished size of the lungs, the information conveyed is of no positive value whatever. This superficial dulness, as it is termed, being of no value in estimating the size of the heart, it is evident that we must have recourse to mapping out the entire area of the cardiac dulness, both superficial and deep, to use for the nonce those anatomical terms which are so constantly applied to the acoustic phenomena, which, as Auenbrugger[1] first pointed out, ought to be explained and described solely by reference to the physical condition of the parts implicated, and not by any reference to their anatomical position, that being a matter not immediately apparent from the sounds elicited, though it may be deduced from them by a process of ratiocination.

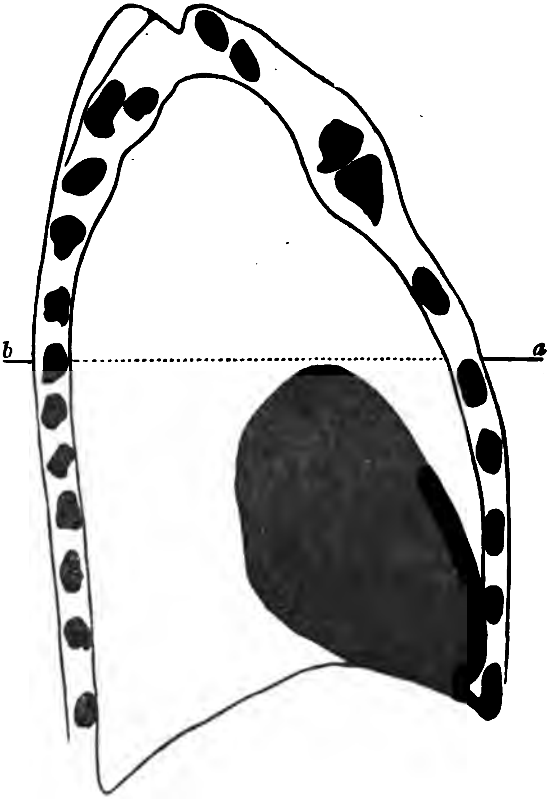
The heart and large vessels occupy the centre of the chest, extending from the upper border of the third rib to the lower end of the sternum, and almost entirely occupying the space between the sternum and the vertebræ. In this position the ventricles encroach more upon the left lung, and the auricles upon the right one, the whole of the right cavities lying anterior and slightly to the right of the left ones; the axis of the right ventricle in relation to the pulmonary artery being almost vertical, the ventricle being broadest at the part most distant from the artery; while the axis of the left ventricle is almost horizontal in relation to the aorta, the ventricle being narrowest at the part most distant from the artery; in consequence of the peculiar formation of each ventricle, and of the anterior position of the right one, the pulmonary artery arises in front and to the left of the aorta. From the formation and position of the heart, it is obvious that, though we can and may percuss out the whole of the cardiac dulness, this is quite unnecessary; it is only of importance to ascertain its greatest extent of dulness vertically and transversely. Increase of the vertical dulness rarely indicates any alteration in the size of the heart itself, but is usually either due to hepatic enlargement, readily ascertained by an extension of these exploratory methods to the liver itself, or to pericardiac effusion; the former dulness, as a rule, extending below the sixth rib, the latter above the third; while a simple change of position of the heart, which may arise from various causes, is indicated by a transference of the normal dulness upwards or downwards, without any change in its extent. The apex beat, except in certain abnormal conditions, is,

Anatomy of the Organs of the Chest, published in the twelfth volume of the "Transactions of the Provincial Medical and Surgical Association." It has been selected because the area of cardiac dulness approaches more nearly in configuration the ordinary conventional idea of such dulness than that of the other figures. How much this may vary within the limits of health may be seen by a reference to the other figures taken from bodies with healthy organs.

[1] Inventum Novum, §§ 17, 18, Scholia.

from the formation of the heart, the part which extends farthest to the left, and, being, as a rule, perceptible to the touch, only requires to be percussed out in those exceptional circumstances where the true apex beats beneath a rib, and not in an interspace. The right auricle is, of course, that part of the heart which extends farthest to the right, and being extremely dilatable, and readily influenced by any obstacle to the onward flow of the blood, transverse dulness about the level of the fourth rib comes to be an important indication of some obstacle to that onward flow, and therefore of enlargement of the heart chiefly in its auricular region. These, therefore, are the chief points in regard to which we look for important information from the percussion of the cardiac dulness. Increase of dulness above the third rib indicates, as a rule, pericardiac effusion. Increase of the transverse dulness at the level of the fourth rib indicates obstruction to the circulation. If the apex beat be displaced to the left and downwards, the obstruction is probably aortic, and has primarily influenced the left ventricle; if the apex beat be not displaced downwards, the obstruction is either mitral or pulmonary in its origin. In percussing the cardiac dulness, we must always remember that, so far as the three sides surrounded by the lungs are concerned, the passage of the percussion note from clear to dull is not abrupt, but transitional, because the heart upon all these three sides is covered by a gradually decreasing layer of lung. Practically, we find, in percussing the cardiac region, that the percussion note from above downwards ranges from clear and full just above the upper border of the third rib to perfectly dull somewhere below the lower border of the fourth rib, the note becoming gradually less full, but still perfectly clear, until it reaches the limit of dulness. As this diminution of fulness depends upon a lessening of the air-space, Skoda has applied to it the term *leer* or empty, the percussion sound becoming gradually emptier by the filling up of the resonant air-space from below, while it may also become gradually duller—more muffled—by filling the air-spaces from above,—that is, by increasing the density of the medium percussed, whose vibrations originate the sound, a perfectly empty sound and a perfectly dull one being, of course, synonymous; the first, however, remaining clear to the last, while gradually becoming emptier until perfect emptiness and perfect dulness coincide; while a muffled or originally dull sound may be full at first, but may gradually become emptier till perfect dulness and perfect emptiness coincide. The first form of dulness, the emptying of the sound, depending upon a gradual diminution of the air-space; the second form, the muffling or dulling of the sound, depending upon the gradually decreasing capacity of the percussed medium for undergoing sonorous vibrations. A percussion sound becomes, therefore, gradually emptier, less full, the duller or more muffled it becomes; but it by no means necessarily grows duller by becoming emptier, because a sound may be very empty, and yet perfectly clear. It is obvious that, in this signification, the terms full and empty are equivalent to greater or less intensity of sound, an intensity in this instance depending upon the quantity of sound reaching the ear. A glance at the accompanying diagram (Fig. 2[1]) will show that, in the normal condition, we have

Fig. 2.



above the third rib (along the line *a b*) a larger air-space—a larger resonance box, and therefore a fuller sound producible on percussion

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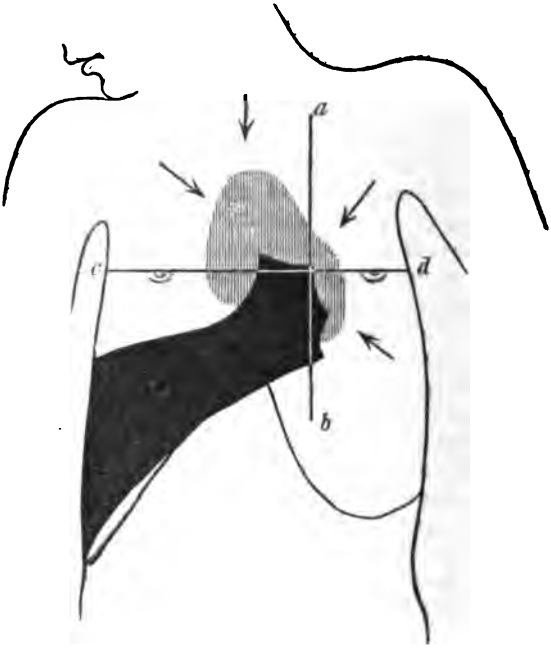
[1] This figure is a diagramatic representation of Fig. 2, Tab. 5 of Fasciculus 2 A of Pirogoff's "*Anatomia Topographica Sectionibus per Corpus Humanum congelatum triplici directione ductus illustrata.*"—Atlas, Petrop., 1859. The section passes at the upper part through the left sterno-clavicular articulation, at three Paris lines from internal margin of the head of the left clavicle. The lowest rib anteriorly is the seventh, cut through ten lines from the sternum; posteriorly, it is the twelfth rib, cut through nine lines from its vertebral extremity. It will be observed that, in this figure, the lung extends lower than ordinarily, and that perfect dulness would not commence till the lower edge of the fifth rib, instead of the fourth rib as usual.

—than below the third rib, and that the air-space gradually lessens, and, consequently, the percussion note gradually becomes less full, as we approach the lower border of the fourth rib, continuing to be clear up to the very last. In most works upon the subject, we are told to percuss more forcibly below the third rib, in order to elicit what is termed the *deep dulness;* but the idea of deep dulness is a composite conception with which acoustics have only a relative connexion; all we require to ascertain is simply the size of the resonance box—the greater or less fulness of the percussion sound; and in the average normal condition of the chest-wall and the contained organs, the slightest tap is sufficient to elicit the difference between a full and a less full or emptier percussion sound. When the chest-walls are denser or less vibratile, and the percussion sound consequently more muffled, a more forcible percussion may be required; but this refers equally to the full and to the less full sound; is the same, therefore, as regards percussion above the third rib and below it.

Acoustic phenomena are generally spoken of in relation to their intensity, pitch, and clang or *timbre.* By many observers the phenomena just referred to intensity, and spoken of as full or empty sounds, have been attributed to pitch, and regarded as low or high sounds; and indeed an emptying of the sound is necessarily accompanied by a heightening of its pitch. Because the length of a column of air which most perfectly resounds to the vibrations of a tuning-fork is exactly equal to one-fourth of the length of the sonorous wave produced by the fork, and this wave diminishes in length as it increases in rapidity, increase in rapidity being equivalent to heightening of pitch, as we learn from experiments on the syren. Hence the shallower any air space, the higher the note with which it most perfectly consonates, a percussion note becoming emptier becomes therefore also higher in pitch. Not that there is any alteration in the general rapidity of the vibrations of the membrane percussed because the resonance-box over which it is stretched is shallow, but because the air space beneath the part percussed selects those flutters with which it consonates, and raises them only to the dignity of an audible tone. Clang, on the other hand, depends upon the mingling of the overtones or harmonics belonging to the vibrating body itself with its fundamental note, and therefore varies with the nature of the structure thrown into sonorous vibrations by percussion—varies, therefore, as we percuss the intercostal tissues, the ribs, or the sternum—varies even with the structure of the pleximeter or plessor employed. Nay, more, clang, mingled with resonance, has its influence in altering the percussion notes in those cases where, from emaciation of the chest-wall and size of the pleximeter, the latter rests upon two ribs, leaving an air-space of varying size beneath. To avoid the confusing influence of clang, and, to some extent, also, that of varying tension or elasticity, in estimating the exact quality of a percussion note, it is advisable, in percussing from above downwards, to compare rib with rib, and interspace with interspace; and, in like manner, in percussing transversely, we must carefully percuss along a rib or along an interspace, and avoid shifting indefinitely from one to the other. Both pleximeter and plessor must be as much as possible free from clang themselves, and the former must also be capable of close and accurate application to either rib or interspace; and in these respects no instruments are comparable to the forefinger of the left hand for a pleximeter, and the first two or three fingers of the right hand as a plessor, the only objection to their use—and it only applies to class teaching—being the comparatively less intensity of the sound produced; but this difficulty is easily surmounted by the use of a Winterich hammer as a plessor, by means of which an adept can, without injury to the finger pleximeter, produce a percussion note of perfectly sufficient intensity. A less skilful party using such a hammer is apt not to tap smartly enough; he may hit hard enough—there is not much likelihood of failure in that respect; but he does not lift the hammer quickly enough, and he therefore muffles or stops the sonorous vibrations it is his object to excite. It occasionally happens that the dulness of the right auricle is not appreciable to the right of the sternum, and under these circumstances the clang of the sternum may be so sonorous as—in percussing transversely—apparently to do entirely away with all cardiac dulness; but, unless under very exceptional circumstances, we can remedy this matter by percussing from above downwards; and by eliciting the sternal note above the aorta, we can at once map out the area of cardiac dulness lying beneath the sternum. We must also never forget that the arch of the aorta does not extend like a bow transversely across the sternum, as Piorry has figured it, but that, as anatomy teaches us, it is in close contact with the pulmonary artery and the heart at every point, so percussion can only map it out as a somewhat rounded projection at the base of the heart. We must also remember that, as a rule, the dulness of the innominate artery is scarcely perceptible, while that of the left carotid, and that of the left subclavian, is altogether imperceptible. For this the clang of the ribs lying over them is partly to blame; but it is, no doubt, chiefly due to what is called the inflection of sound, by which a sonorous wave embraces a non-resonant body on all sides, and may, so far, extinguish the dulness of a small one as to render it inappreciable. Hence, marked dulness in these regions is usually significant of some considerable morbid alteration.

As the heart rarely rises above the level of the third rib, and if it does so, it rises, as a rule, equally on both sides, so it is obvious that Piorry's line of oblique dulness, from apex to base, is of no special practical value; and, from all we have said, it follows as a corollary that there are only two lines of percussion of any practical importance—viz., a vertical and a transverse one. The first of these must be thrown so far to the left as to be uninfluenced by the aorta and the pulmonary artery, and I have been in the habit of placing it at one inch to the left of the left edge of the sternum. When, percussing from above downwards from the lower edge of the left clavicle, we have in the normal condition first the clear, full sound of the lung down to the upper border of the third rib; beneath that, a gradually emptying but clear sound down to the lower border of the fourth, or upper border of the fifth rib; and, beneath that, perfect dulness till we reach the tympanitic sound of the stomach or intestine. Any deviation from this is abnormal, and its explanation must be sought by other methods of investigation. In like manner, the transverse dulness in the line of the fourth rib, which is that mainly affected by distention of the right auricle, must be carefully mapped out, percussing from without inwards on each side—that is, from the lung to the heart; because in that way alone we can best appreciate comparative shades of dulness, and also eliminate the clang of the sternum. The accompanying diagram (Fig. 3[1]) exhibits the percussion dulness of the normal cardiac area, with the lines of practical importance (vertical, *a* to *b*; transverse, *c* to *d*) specially indicated. To separate the lower part of the heart from the liver is by no means an easy matter. It is said that we can occasionally

FIG. 3.[1]



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[1] This figure refers to the same subject as Figure 1. Percussion is to be made in the direction of the arrows.

make out a line of clearness between the two. I have never observed any such line of clearness, and, as the heart invariably rests upon the diaphragm, unless separated by fluid, I do not clearly see the mechanism by which this line of clearness may be produced. Any change of pitch between the cardiac and hepatic dulness is impossible, and can only be produced by upward conduction of the tympanitic note from the stomach or intestines. It follows, then, that we can only separate the heart from the liver approximately, by ascertaining the position of the apex beat upon the left side, and also the highest point of hepatic dulness upon the right side, and joining the two by a straight line. This line of separation can never be absolutely accurate, but is usually sufficiently so for all practical purposes.

It is sometimes of importance to map out the aortic dulness, so as to ascertain if it be increased in any direction; and this is by no means a difficult task, if we set about it in a right way. As I have already said, it is impossible to separate the aortic dulness from that of the heart: the only part in which this might be possible is occupied by the pulmonary artery, the dulness of which, however, differs in no respect from that of the aorta. It is therefore absurd to map out the aortic dulness, as Piorry and his followers have done, as an independent arch crossing the chest above the area of cardiac dulness. The normal aortic percussion is indicated in the foregoing diagram (Fig. 3), in which it will be seen to occur simply as a more or less rounded bulging, rising out of and inseparable from the general area of cardiac dulness, and only known to be aortic from its position above the vertical limit of cardiac dulness as indicated by the upper limit of the dulness in the line *a b*. Increase of this dulness in any direction may depend upon morbid enlargement of the aorta; but in the diagnosis of these enlargements, percussion dulness is only one of many physical facts which require to be collated and reasoned upon in estimating the probabilities for and against the assumption of the existence of any such morbid condition.

Absence or diminution of the precordial dulness in the rarest of cases is caused by the presence of air in the pericardium, less seldom by atrophy of the heart, and infinitely more frequently by increased extent of the pulmonary resonance from emphysema. Increase of the area of cardiac dulness is a much more common phenomenon. This may depend upon effusion into the pericardium, in which case the dulness is pyramidal, with the base downwards; and in this case the cardiac pulsation is annulled, enfeebled, or displaced. Or the increased dulness may depend upon enlargement of the heart, with or without hypertrophy; in these cases, the pyramidal shape of the dulness is less marked, but its broadest part is always above. When hypertrophy predominates, the cardiac shock is increased; when dilatation predominates, it may be almost annulled. But we must never forget that these conditions may be variously modified, and that our diagnosis, to be correct, must be based, not upon one or two facts, but upon all the phenomena which can be ascertained in regard to the physical condition of the heart.

The next and last method[1] which we can employ in ascertaining the physical condition of the heart is AUSCULTATION, by which we understand the art of ascertaining the physical condition of the mechanism of the heart by the sounds produced during the passage of the blood through its several cavities.

If we place our ear over the cardiac area during the progress of the circulation, we become conscious that it is accompanied by sounds alternated with silences. If we listen over the heart of an infant, we distinguish only a uniform ticking, in which the sounds are alike in intensity, and the silences similar in duration; but, as we gradually extend our experience by listening to the hearts of older individuals, we find that, as we approach adult life, these sounds, with their intervening silences, assume a peculiar rhythm, which the ear thus educated very readily appreciates; and we also discover, that while the silences remain the same in every position, the sounds vary somewhat, according to the part of the cardiac area in which they are heard. Thus, when listening over the apex beat, we distinguish two pauses or intervals of silence,—a long pause and a much shorter one; immediately succeeding the long pause, we hear a dull, prolonged sound, followed by the short pause, and this succeeded by a much shorter and sharper sound, which is immediately followed by a renewal of the long pause, the accent in this situation being upon the prolonged or first sound following the long pause, making what is called a trochee in prosody—"– ⏑." On the other hand, when we listen over the base of the heart, we find that, while the relation of the sounds to the silences remains the same, the accent in this situation falls upon the second sound instead of on the first, so that, following the long pause, we have, instead of a trochee, an iambus—"⏑ –." We see thus, that though the cadence of these sounds alters with the position in which they are heard, they can, in the normal adult, be readily enough recognised to be first and second by their relation to the periods of silence; but whenever the heart's action becomes rapid from fever, debility, or any other cause, the long pause is diminished, and the rhythm of the sounds approaches that of the infant, so that, when the pulse beats more than 90 times a minute, it is almost impossible to distinguish with accuracy what is first and what is second, and if for any reason it is desirable to do so, we must employ a double stethoscope, and by placing one end over the heart's apex, and the other over the base, a little practice and a little care will enable us readily enough to distinguish what is first from what is second, by a due attention to the slight difference in the character of the sounds, and by a careful appreciation of the position in which each sound is most distinctly heard. Apart from the alterations in rhythm, which are due to the rate of cardiac action, the heart sounds vary in distinctness in each individual; and experience has taught us, that in nervous hearts, which are usually thin-walled, and in dilated hearts, which are always thin-walled, the first sound is particularly loud, clear, and distinct; while, in hypertrophied, and therefore thick-walled hearts, the same sound is always muffled and indistinct, the second sound not being affected by this cause. Of course we cannot securely base our diagnosis of the state of the cardiac walls upon the distinctness or indistinctness of the sounds; but this phenomenon supplements and confirms the information we obtain in other ways. The second sound also varies in distinctness and in intensity, but always from causes which are extracardiac.

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[1] For, of course, in the practical exploration of the heart, we exclude both the cardiograph and the sphygmograph, the use of which can never become general as a means of clinical examination by an ordinary practitioner.

Besides the alterations in rhythm depending upon alterations in the rate of cardiac action, and alterations in distinctness depending upon hypertrophy, dilatation, or some extracardiac cause, we find these sounds occasionally replaced by noises, *bruits*, or murmurs, as we term them, which are totally different from the sounds they displace, and which vary with their causes.

Physiologists teach us that the first sound coincides with the contraction or systole of the ventricles, and is composed of several ingredients, of which the shock of the heart's apex on the walls of the chest, the bruit which accompanies muscular action, and the sound produced by the closure of the auriculo-ventricular valves, are the most important. But for all practical purposes the last is all-sufficient, as we find that whenever these valves are from any cause —such as dilatation of the ventricle—rendered incapable of closure, the valves themselves being healthy, the first sound is almost entirely replaced by a murmur which more or less completely obscures the muscular bruit, and, to a less extent, the shock. The second sound, on the other hand, as it immediately follows the ventricular systole, must coincide with the ventricular diastole, and therefore with the moment of time when the arterial semilunar valves are closed by the recoil upon them of the blood compressed by the arterial systole; and, accordingly, we find that when these valves are from any cause rendered incapable of closure, the second sound is more or less completely replaced by a murmur according to the degree of imperfection present.

The heart, however, is a double organ, with two auriculo-ventricular openings and two large arteries springing from it; and it is of importance to determine which side is affected. But it so happens that all the cardiac valves lie so close together, that a superficial area of half an inch square will include a portion of all the four sets of valves *in situ*, while an area of about one quarter of an inch will include a portion of all except the tricuspid.[1] It is obvious, therefore, that it is impossible to differentiate the sounds or murmurs produced by one valve from those produced by another by merely listening with an ordinary stethoscope over the place of origin. By the rhythm alone we can readily distinguish which is first and which is second; but to differentiate a right first or second sound from a left first or second sound, we must take means to separate the one from the other; and this we do by taking advantage of the facts that sounds produced in one medium lose in intensity in passing into another, and that sounds produced by any fluid in motion are invariably transmitted in the direction of the onward current.[1] Hence we find that sounds produced in any one cavity of the heart are heard with most distinctness over that part of the thoracic wall at which the given cavity approaches the surface most closely; thus, the only point at which the left ventricle directly impinges on the chest-wall is where the apex beat is felt, and that is precisely the spot where the first sound produced in the left heart by the closure of the mitral valve is most distinctly heard: a space of about an inch in diameter around the apex beat is therefore termed the mitral area. Nearly the whole of the right ventricle is uncovered by lung, and impinges directly on the lower part of the sternum; and at this part, especially along the left edge of the sternum, where it is joined by the cartilages of the fourth, fifth, and sixth ribs, the right first sound produced by the closure of the tricuspid valve is best to be heard, and the triangular space covering the position of the right ventricle is therefore termed the tricuspid area. In the normal condition, the two ventricles act simultaneously; and the two sounds differ so little from one another, that it is impossible to differentiate clearly the one from the other. When, however, from any cause, these sounds become irregular or replaced by bruits, the differentiation by means of a reference to these areas of audition is readily enough made, though, of course, there are always plenty of confirmatory proofs.

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[1] Walshe on Diseases of the Heart, 3d edition, 1862, p. 6.

On the other hand, there is, both in health and in disease, a marked difference between the aortic and pulmonic second sounds, and it is often of the greatest importance to differentiate the one from the other, and clearly to recognise the distinctive characteristics of each. As already mentioned, the aorta and pulmonary artery originate very close to each other, the pulmonary valve lying about the middle of the third left cartilage, one half being to the left and the other to the right of the left edge of the sternum, which exactly divides it in two. From its point of origin, the pulmonary artery rises to the lower edge of the second left cartilage, where it divides into its two great branches going to the right and left lungs; this, therefore, is the place where it is nearest the surface of the chest, and the second interspace, or the sternal end of the third cartilage, is the position in which the pulmonary second sound is best heard. On the other hand, the aorta rises a little below, behind, and to the right of the pulmonary artery, its valves corresponding to the lower edge of the third left cartilage, behind and to the right of the pulmonary valves, and it passes upwards, forwards, and to the right, till it reaches the upper border of the second right cartilage, when it passes obliquely backwards and to the left, forming what is termed the transverse portion of the arch of the aorta. At the second right cartilage, therefore, the aorta is nearest the surface of the chest, and arterial walls and the blood-current coincide in readily conveying in this direction the resonant vibration which results from the closure of the aortic semilunar valves; in this position, therefore, the aortic second sound is most readily differentiated from the pulmonary one. In the normal condition the aortic second is heard to be louder and more distinct than the pulmonic one; both vessels are, however, normally covered with lung, and as from various causes, congenital or morbid, one or other may become uncovered and thus brought nearer the surface, its corresponding sound may then be heard more distinctly than is normal, and the ordinary condition may then be either intensified or reversed; but this does not happen without the occurrence of other phenomena which enable us to detect and explain the source of the abnormality, as we shall by-and-by have occasion to see. But besides this greater distinctness of the one sound more than the other, depending upon mere alteration in the anatomical relation of the parts, we have occasionally an alteration in the character of one or other of these sounds depending upon intrinsic causes. To this peculiar alteration of the second sound a variety of terms have been applied: it has been called booming, ringing, clanging, pumping, cavernous, and accentuated; and, perhaps, accentuated or booming are the most unobjectionable expressions we can employ to define this particular change in the second sound, in which the element of tension seems united with an increase of distinctness—a combination which, however, must be heard to be properly understood. This accentuation of the second sound may be heard either at the second right or at the third left costal cartilage; it may, therefore, be either of aortic or pulmonic origin. Its occurrence in the pulmonary artery was long ago pointed out by Skoda as an important aid in the diagnosis of mitral disease; but it is not only in mitral disease that the presence of an accentuated pulmonary second is of important significance, for this accentuation is constantly present in every form of cardiac disease involving obstruction to the onward flow of the blood, and is the most persistent of all the acoustic phenomena indicative of cardiac disease, being frequently the only thing markedly abnormal to be detected. In the absence, therefore, of any pulmonary disease capable of producing congestion, persistent accentuation of the pulmonary second sound is to be regarded as invariably indicative of cardiac valvular lesion. The mode in which this accentuation is produced is very simple. The pulmonary circulation is a closed circulation, and, like the water in a Bramah press, the blood within the pulmonary circuit presses equally in all directions; any obstruction to its onward flow, therefore, produces not only congestion of the lungs, but also a uniform increase of the tension throughout the whole of the pulmonary circulation, often accompanied, if long continued, by slight dilatation of the pulmonary artery, and always by a closure of the semilunar valves with an exaggerated force proportionate to the hindrance it has met with. In the aorta the case is somewhat different, but the explanation of this is equally simple. The systemic circulation, though also anatomically a closed circulation, is yet incapable of acting like a Bramah press, because of the many local conditions which may influence it in various ways. The portal circulation itself cuts off a varying yet always considerable amount of blood from any participation in general hydraulic influences; while each of the extremities, as well as every gland, has a circulation which, the experiments of Claude Bernard have taught us, may be entirely separated as regards acceleration or retardation—congestion of veins or arteries—from the general circulation, by means of local nervous influence. It is quite otherwise with the lungs. Here we have solely to do with the circulation through two large glands, whose circulation is so united as to make them in that respect but one, so that though nervous influence may either accelerate or retard this through the lungs generally, or through only a part of either of them, as in the case of local inflammation, yet the circuit is so short, and the vascular connexion so intimate, that this influence is not merely felt locally, but is exerted on the pulmonary circulation generally, though it may vary in degree according to circumstances. The result, however, of the systemic circulation being compounded of many local circulations in which the blood tension is continually varying, is completely to neutralize the effect of these local alterations on it as a whole; while the variety of causes which may produce these alterations, and the variety of times and positions in which they act, is such as to render any general alteration impossible. Accentuated closure of the aortic semilunar valves, from a general or systemic cause, is therefore not to be thought of; it can only arise from some local cause. But the only local causes possible are, first, increase of arterial tension, which, to be effective in this matter, must be general; and this, as we have just seen, is impossible. And, second, increase of the weight, and consequently of the volume of the blood closing the valves, irrespective of the arterial force acting upon it; and as increased amount of contents involves increased size of what contains it, we arrive at dilatation of the ascending aorta chiefly, and partly, also, of the innominate artery, as the sole active causes in producing accentuated closure of the aortic semilunar valves. Accentuation of the pulmonary second, unaccompanied by disease of the lungs, is therefore an invariable

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[1] Sound is reflected, inflected, and refracted like light, and is readily conveyed to almost any distance in smooth tubes; the difficulty which it finds in passing from one medium to another, even though these should be only strata of the atmosphere of varying densities, is sufficiently indicated by the curious statement that battles have been lost for want of reinforcements which were actually waiting within what was thought to be earshot for the sound of the cannon to indicate the moment of advance.—*Vide* Tyndall "On Sound," London, 1869, p. 23, etc.

sign of some cardiac lesion; while accentuation of the aortic second is as invariable a sign of aortic dilatation, when these accentuated sounds are heard in their normal positions, the third right and the second left costal cartilages.

Outside of the normal area of cardiac dulness, as depicted in Fig. 3 (p. 1072), we occasionally hear the normal cardiac sounds more distinctly than is usual, and this may depend either upon increased resonance from condensation of that part of the lung over which they are heard, or upon an aneurismal bulging of the aorta at that part. In the latter case, we may either hear the normal cardiac sounds louder than usual in the given position, or if the cardiac sounds be abnormal, these abnormal sounds may be heard more distinctly than they ought to be; or the second sound alone may be heard accentuated in this abnormal position, or this abnormally placed accentuated second sound may be preceded by a localized systolic bruit, or it may be more rarely replaced by a localized diastolic bruit. But all these sounds, when heard outside of the normal area of cardiac dulness, indicate disease of the lungs or of the aorta, if they be identical with those heard within the cardiac area; if they vary from them, then they indicate disease of the aorta in one or other of its various forms, and in any case have only to be noted for further examination. In the normal condition of the heart, the sounds and silences succeed one another in the manner described,—each ventricular systolic sound being accompanied by an arterial pulse perceptible in the radial arteries. But it sometimes happens that these sounds and silences succeed one another with perfect regularity over the cardiac area, and yet the radial pulse intermits; this is an evident sign of cardiac debility, and must be noted, that its cause may be inquired into. At other times the intermission extends to the cardiac action itself, and this also must be noted for further inquiry. At still other times the cardiac sounds and silences, and also the radial pulse, occur with extreme irregularity; and this also must be carefully noted, that its cause may be subsequently inquired into.

But there is still another form of irregularity in the cardiac sounds which may be a symptom of disease, but which may also occur in perfect health, and that is reduplication of these sounds. In its most extreme and rarest form we have, instead of two, four sounds,—two first and two second sounds; the two beats acting separately and not simultaneously as usual. More usually, however, we have three sounds instead of two; and the rarest form of this is when the first sound is reduplicated, which is extremely seldom to be heard with any distinctness. A reduplication of the second, however, is a matter of common occurrence not only in disease but even in health. It is produced by a more rapid action of one ventricle compared with that of the other, and is heard as a *bruit de rappel* either at base, apex, or both. It may be vocalized by the syllables ta-ta, and closely resembles the sound of a hammer which strikes the anvil, rebounds, and strikes again, remaining motionless.

Reduplication of the second sound is of frequent occurrence in m constriction where there is a physical cause capable of preven the filling of the left ventricle, and which therefore permits the ri one to get the start of it. Reduplications of the cardiac sounds disease are often permanent; in health they are more frequen fugacious, appearing one instant and disappearing the next. Th are merely abnormal exaggerations of a phenomenon which, w care and appropriate appliances, may be detected in every on reduplication of the first sound occurring at the end of the expiration or commencement of the inspiration; reduplication of second sound occurring at the end of the inspiration and commencement of the expiration. These normal reduplications depend upon the variations in the pressure produced by the respiratory movements at the origin of the arterial and venous systems. The first sound becomes reduplicate because excess of blood-pressure in th heart retards the closure of the tricuspid valve; and the second soun is reduplicated because excess of pressure in the aorta accelerates th occlusion of the aortic valves.[1]

Within the cardiac area, as already remarked, murmurs may take the place of one or other of the normal sounds, or may occupy the time of one or other of the silences, or may even occupy the whole period of a cardiac pulsation, or any portion of it. Such murmurs may be of endocardiac or of exocardiac origin, and as endocardiac murmurs are most frequent, and are chiefly due to valvular lesions, it is a good rule to endeavour first of all to associate all murmurs with a valvular origin, and only after exhausting this hypothesis to proceed to determine the probabilities in favour of their extracardiac origin, or of their dependence upon some intracardiac cause apart from valvular lesion.

Of all the signs of cardiac disease, murmurs are those most usually confided in, and yet they are really those of least value; first, because, as already remarked, exocardiac murmurs may simulate those of endocardiac origin; and, second, because murmurs truly of valvular origin may disappear temporarily or permanently.[2] Thus we may have murmurs apparently of valvular origin which are really exocardiac in their origin; second, we may have murmurs truly of valvular origin, yet without permanent valvular lesion, which may disappear, leaving the heart uninjured; and, lastly, we may have murmurs truly of valvular origin which may disappear temporarily or even permanently, the valvular lesion still continuing. It is obvious, therefore, that murmurs cannot of themselves be accepted as certain indications of cardiac disease, even although we can positively connect them with the lesion of a definite valve, because that lesion may be temporary in its character and wholly unconnected with actual disease of the valve affected. To this there is but one positive exception, and that refers to the auriculo-systolic, the so-called presystolic murmur; though there is obviously a greater or less probability of any of the other valvular murmurs being also connected with actual disease of the valves affected. That probability, however, falls to be considered under the head of each separate valve; at present I have only to describe the acoustic phenomena audible on auscultation, to point out the means by which we determine whether these are valvular in their origin or not, and to state that no murmur, except the one already referred to, can ever be accepted as a definite sign of actual cardiac disease, but must be simply noted, to be afterwards duly considered along with the other information derived from inspection, palpation, and percussion, when we come to estimate the probabilities for or against the existence of any special lesion, for there is no royal road to diagnosis any more than to anything else that must be made, as Opie mixed his colours, "with brains."

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[1] Potain, "Note sur le Dedoublement Normaux des Bruits du Cœur."—*L'Union Médicale*, 1866.

[2] On the Variation and Vanishing of Cardiac Organic Valvular Murmurs, by W. R. Sanders, M.D., Professor of Pathology in the University of Edinburgh.—*Ed. Med. Journal*, Jan. 1869, p. 584.

When we hear, over any part of the area of cardiac dulness, any sound which differs from those sounds we have already taught ourselves to recognise as those audible in the normal condition of the heart, our first care must be to ascertain by careful examination at what part of the cardiac area this peculiar sound is most distinctly to be heard; this we may term the position of maximum intensity. This position either coincides with one of the normal areas already described as the mitral, the tricuspid, the aortic, or the pulmonary area, or it does not. If the position of maximum intensity of this abnormal sound coincides with one or other of these normal areas, it most probably depends upon some lesion, temporary or permanent, of that valve whose normal sound is audible in that position, and which it either obscures, replaces, precedes, or follows. If it does not coincide with one or other of these normal areas, it is certainly not valvular in its origin, unless this position of maximum intensity be over the lower end of the sternum, or just to the right of the pulmonary area and in the same plane; and that any such murmur may be of valvular origin, it must be diastolic in the first position and systolic in the latter; the reasons for this being to be found in the anatomical relations of the parts, and in the mode in which sound is conducted.

Our next care must be to determine the actual rhythm of the murmur or abnormal sound, that is, its positive relation to those several physiological acts which constitute a cardiac pulsation. That we may be able to do this, we must first of all have a clear conception of those physiological acts which make up what we call a cardiac pulsation; and, secondly, we must be able to recognise them. The accompanying figure (Fig. 4, altered from Gairdner[1]) represents diagramatically the several acts of a cardiac pulsation in the normal adult heart, in which a long sound precedes a short silence, followed by a short sound, and that again by a long silence. When the heart beats under 90 per minute, there is never any difficulty in making out the several relations of these sounds and silences, however slow the heart's action may be. When, however, the heart's action is over 90 per minute, it approaches more or less to the uniform tic-tac of the infant's heart, and the more nearly this is the case the more equal are the two silences, and the greater is the difficulty experienced in determining what sound is long or first and what is short or second. To ascertain this with precision, we must either wait until rest and other agents have slowed the heart's action to 90 or fewer beats per minute, or we must depend upon our accuracy in the use of a double stethoscope, knowing that under all circumstances the first sound is always relatively long at the

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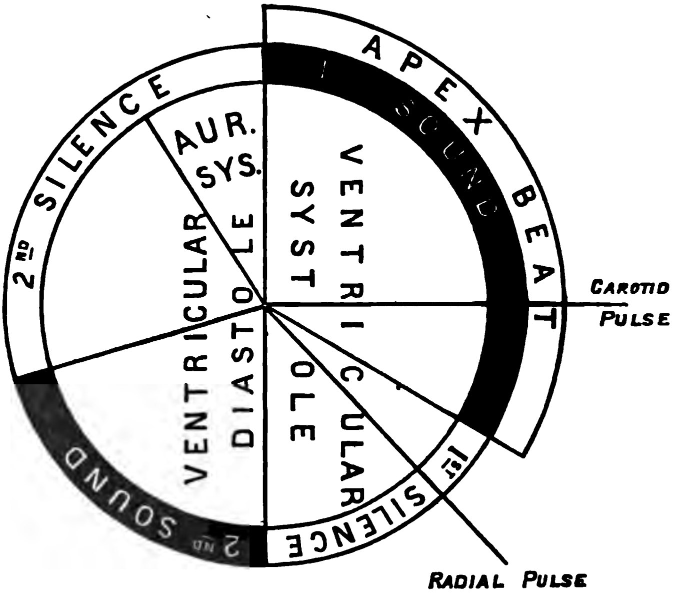
[1] To Dr Gairdner we owe the first attempt to represent a cardiac pulsation diagramatically. This diagram, which is altered from his, is merely an attempt to give more physiological accuracy to it. The apex beat and the two sounds

FIG. 4.



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are represented as occupying, as they do, appreciable intervals of time during that pulsation. The heart's action is represented as continuous and without actual pause or rest, though at times soundless, while the carotid pulse is placed in the position which Valentin says it ought to occupy in relation to the commencement of the apex beat, and the radial pulse is placed in more nearly its usual relation both to the carotid pulse and the apex beat. The foot pulse is nearly synchronous with the radial. According to Mr A. H. Garrod, the difference between the two, with a pulse beating 75 per minute, amounts to 0·0012 of a minute.

apex and short at the base, while the second is exactly the reverse; or we may determine what sound or murmur coincides with the apex beat or carotid pulse, that being certainly coincident with the ventricular systole; remembering that any reference to the radial pulse is irrelevant and misleading, as even in the normal condition it is separated from the apex beat by an interval which may amount to about one-sixth of a second, and that this interval may be increased in disease to the half, or even to the whole period of an entire cardiac pulsation.

Having determined the position of maximum intensity of the murmur, and ascertained its rhythm, we are prepared to state the nature of the lesion upon which it depends. All that we require to do this is simply to remember that in the normal condition the auriculo-ventricular valves on both sides are closed during the systole of the ventricles, the arterial valves on both sides being opened; while during their diastole the reverse takes place, the arterial valves are closed and the auriculo-ventricular valves are opened. Thus a murmur, whose position of maximum intensity is in the mitral area, depends upon some defect in the mitral valve: if it be synchronous with the systole of the ventricles, it depends upon some defect in its closure, and is therefore a murmur of regurgitation backwards into the auricle; but if it be synchronous with the diastole of the ventricles, it depends upon some obstruction to the flow of the blood through the open mitral valve, and is therefore a murmur of obstruction. The first of these murmurs runs off from the apex beat, and more or less completely replaces the normal first sound. The second murmur accompanies or follows the second sound, but in no respect interferes with its production. That murmur, or portion of a murmur—for the murmur may be continuous—which follows the second sound by an appreciable interval, and whose position of maximum intensity is not only placed in the mitral area, but which is but little audible out of that area, immediately precedes and runs up to the first sound, (is therefore presystolic); but a glance at the diagram (Fig. 4) shows us, what we indeed already know, that the systole of the ventricles is immediately preceded by the systole of the auricles, and that what we term presystolic is truly auriculo-systolic in its rhythm. This murmur is not only a murmur of obstruction, but a murmur of obstruction to direct cardiac action, and, like all such murmurs, is always rough. It is the only cardiac murmur invariably associated with actual disease of the valves affected.

These three murmurs may exist separately, as occasionally they do, or any two of them may coexist; and sometimes the whole three are present, and then we have a murmur audible in the mitral area, and running through the whole period of cardiac action, the first sound being either much altered or entirely replaced; while the second sound, though not always audible at the apex, always exists, the pulmonary second being greatly accentuated, unless the two second sounds are heard separately, the one immediately following the other, as what I have already mentioned as a reduplication of the second sound, in which case the accentuation of the pulmonary second is always modified.

These murmurs, though most common on the left side, may also be heard over the right heart, a systolic tricuspid murmur being very frequent, a presystolic tricuspid being somewhat rare, while a diastolic tricuspid is a possible, but, so far as I know, a hitherto unrecorded complication. And in saying this it is obvious that I mean a diastolic murmur over any part of the rigid auricle or ventricle, both the aortic and pulmonary second sounds continuing audible, and with entire absence of any symptom or sign of aortic regurgitation.. This is a most important point, because it often happens that a diastolic murmur, originating in defective closure of the aortic valves, is heard loudest over the lowest part of the sternum, being conducted downwards partly by the descending current of the blood, and partly by the peculiar resonance of the sternum. We shall afterwards see that a diastolic murmur, of purely aortic origin, may have its position of maximum intensity over the lower part of the sternum—the tricuspid area, or in the mitral area, and that this depends upon the anatomical nature of the lesion and has an important influence upon its prognosis, being always accompanied by absence or alteration of the aortic second sound, and, if uncomplicated, free from any of the symptoms and signs of mitral or tricuspid lesion, affording a well-marked example of the importance of looking on a murmur as merely one of many physical signs which must all be duly considered in estimating the probabilities in favour of the existence of any given lesion.

At the base of the heart the audible murmurs are apparently less complicated than at the apex, because here we have no presystolic murmur, the only possible murmurs being systolic and diastolic. But the causes of these are so various that the pulmonary region has somewhat deservedly received the name of the region of romance, from the various theories propounded in explanation of the murmurs occurring there; while the physical causes of aortic murmurs, and the physical relations of the aorta itself, are so complicated, and so efficiently modify the conduction of these murmurs, that though it is quite usual to say that a systolic murmur having its position of maximum intensity at the sternal end of the second right costal cartilage is due to aortic obstruction, yet this is by no means always the case; and in like manner, though a diastolic murmur audible in that position is most probably due to aortic regurgitation, yet its absence from that situation is no proof of the absence of regurgitation, because the murmur due to regurgitation may be only audible beneath the level of the aortic valves.

The important thing for us to remember is, that in noting the characteristics of a murmur the chief points to be determined are, *first*, the position of maximum intensity, as probably indicating the valve affected; *second*, the rhythm of the murmur, as indicating whether it is systolic or diastolic; and, *third*, whether the valve sound normally heard in any given position is wholly replaced by the murmur, or only altered in duration and character. We must also remember that even the existence of a well-defined murmur is not by any means a certain indication of actual valvular disease, any more than is the absence of any murmur to be regarded as a sure proof of the non-existence of valvular disease. The characteristics of a murmur, and its relations to the normal sounds and silences, must merely be noted and afterwards duly considered in connexion with the other physical signs already commented on.

Endocardiac murmurs of other than valvular origin are occasionally, though rarely, observed. Sometimes these are due to congenital deficiency of some part of the septum between the two hearts; and in that case they only intensify those valvular murmurs which almost invariably coexist, and extend their area of audibility; or where no valvular lesion coexists, as in other endocardiac murmurs of still rarer occurrence, said to depend upon the existence of some vibrating substance, as a flake of lymph, upon one of the *chordæ tendineæ*, in an otherwise healthy and normal heart, the position of audition and the rhythm may be certainly the same as those of truly valvular origin, from which they can only be differentiated by a due consideration of the other physical signs; that is, by a judicious reference to those physical facts which, as I have already shown, indicate actual obstruction to the onward flow of the blood, and which point out with a certainty, the attainment of which is exactly proportionate to our skill in interpreting these signs, whether that obstruction is substantive or only dynamical. Exocardiac murmurs frequently simulate in their timbre those of endocardiac origin, and when they also coincide in rhythm with the systole or diastole of the heart, it may be difficult to distinguish such a murmur from an endocardiac one of non-valvular origin. As, however, exocardiac murmurs depend chiefly upon the friction of the roughened visceral and parietal portions of the pericardium upon one another, or of the pericardium upon the pleura, and as, unless under very exceptional circumstances, this friction is always greatest over the centre of the heart, and neither at its apex nor exactly at its base, we have a pretty safe indication of the exocardiac origin of the bruit in the non-coincidence of its position of maximum intensity, with the usual situations of that position in the case of murmurs which are of valvular origin. We have also the localization, more or less complete, of the bruit, which never passes to any distance from the area over which it is most audible, and which is not propagated by any blood-current, but simply radiates equally all round the position where it originates, usually possessing a distinctly superficial character; and we have the entire absence of any pulmonary accentuations, or of any anatomical indication of obstructed circulation. The sound produced, also, as a rule resembles the crackling of parchment, the creaking of new leather, or a grating or mere rubbing sound, of greater or less intensity, but only very rarely in any respect simulates the blowing murmurs of valvular lesions. Whatever may be its timbre or its rhythm, it must, like all and every other sound heard over the cardiac area, be carefully noted as to its character, position of maximum intensity, rhythm, degree, extent, and direction in which it is propagated, to be afterwards submitted, along with the other facts ascertained, to a careful and discriminating scrutiny. The same may be said of those murmurs which are audible in various positions out of the cardiac area, and which owe their origin to the passage of the blood through the vessels. Such murmurs are sometimes heard in the arteries and sometimes in the veins. They are chiefly systolic, but sometimes diastolic, in rhythm. In the arteries these murmurs may be strictly localized, or may be audible over every part of the arterial system. In the veins, as a rule, they are audible everywhere, provided certain conditions are complied with, though they are permanently present in certain parts of the body where these conditions naturally exist. Such intravascular murmurs are of the most various significance, and can only be most briefly referred to now. Sometimes they signify serious lesion of the vessels themselves; at others, nothing worse than some slight deformity of the chest, the result of rickets; and at still other times, only an alteration in the condition of the blood itself, which may arise from the most various causes.

I cannot conclude this lecture without a cursory reference to the manner in which these murmurs are believed to be produced, because this is a subject in regard to which practical experiment may yet become a most fruitful source of improvement in diagnosis. The crude ideas of our forefathers, which have culminated in the notion that such murmurs are produced by the friction of the blood-current on its containing walls—an idea which the researches of Poiseulle and others have set aside, by showing that no such friction exists—have been replaced by a recurrence to the theory, first started by Corrigan, that such murmurs are due to the vibrations induced by eddies in the blood-column itself, a theory which has been reduced to scientific simplicity by M. A. Chaveau,[1] who has with great probability referred the causation of all murmurs, whether intracardiac or intravascular, to the sonorous vibrations of fluid veins, such as have been observed by Savart, and which are capable of being transferred, as Marey has experimentally shown, to the surrounding fluid within which the vein may be produced, and to the walls containing it. The roughness of the murmur and the tactile perceptibility of the vibrations are in some degree proportionate to the force exerted in producing this sonorous vein, while its musical character must of course depend upon the rapidity of these vibrations.

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[1] *Comptes Rendus de l'Académie des Sciences*, 1858: "Toute veine fluide est le siège de vibrations susceptibles de produire des sons, vibrations qui ébranlent aussi l'orifice d'ecoulement de la veine."—P. 841. The intravascular murmurs

But the exact manner in which these sonorous veins are produced, and the mode in which the production and conduction of their sonorous vibrations may be modified by evident or imperceptible alterations in the heart's action itself, and in the position of the patient himself, are problems which yet remain to be defined, and an accurate experimental investigation of these problems will certainly prove to be of the utmost importance in the accurate diagnosis of cardiac and vascular diseases.

of anæmia are due "aux vibrations de la veine fluide intravasculaire, vibrations qui se produisent toujours quand le sang pénètre avec une force suffisante dans une partie réellement ou comparativement dilatée de l'appareil circulatoire."—P. 933.

Felix Savart has described the phenomena presented by a liquid vein of water flowing through a circular orifice in the bottom of a vessel. The part of the vein *a b*, Fig. 5, is steady and limpid, presenting the appearance of a solid rod, which obstructs vision and wets the finger when passed through it. This decreases in diameter till it reaches a point (*b*) of maximum contraction, beyond which it is turbid and unsteady, marked by periodic swellings and contractions, does not wet the finger when passed through it, and does not obstruct vision, even though the fluid be mercury. At *b*, in fact, the liquid is no longer continuous, but resolves itself into a series of liquid spherules, which have an appearance of turbid continuity, from their rapid succession never permitting the primary impression wholly to fade from the retina till succeeded by a second. Fig. 6 represents this fluid vein illuminated by an electric flash, when the drops of which it consists are seen as it were motionless in the air, and the cause of the periodic swellings and contractions are at once apparent; for these spheroidal drops, when first detached, have their long axis vertical; when abandoned to their own molecular forces they seek to become spheres, and, like a pendulum in motion seeking to return to rest, the contraction goes too far, and they become flattened spheroids, which again elongate vertically; hence the appearance of alternate swellings and contractions. Savart traced the production of these pulsations to the orifice through which the vein passed, but did not regard them as the result of friction. Under moderate pressure they succeed each other sufficiently rapidly to produce a feeble musical note, the pitch of which may be fixed by allowing the drops to fall upon a stretched membrane.—*Vide* Tyndall "On Sound." London: 1869: p. 244, etc.

Fig. 5.

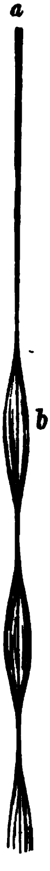
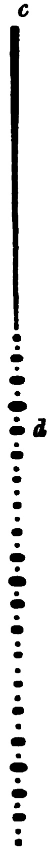


Fig. 6.



ARTICLE II.—*Pelvimetry: A Method of Estimating the Conjugata Vera.* By J. R. HARDIE, M.B., etc.

AUTHORS are generally agreed that in cases of contracted pelvis it is the conjugata vera to which the attention of the accoucheur is to be specially directed. Here, as a consideration of one of the most elementary mechanical problems connected with the development of the female pelvis indicates, we are likely to meet with the evil results of a departure from a healthy condition of the bones during the growth of the skeleton; here, too, there is little room to spare, and any diminution in size, even though ever so slight, is of more serious moment to the subject of it than an equal encroachment on any of the other diameters of the pelvis; besides, both extremities of this diameter would appear to be a seat of election of growths of various kinds, producing a contraction of its dimensions. Small wonder, then, that many have been the attempts to introduce methods having for their object the ascertaining during life of the size of the conjugate. Of these, finding out the external conjugate or the diameter of Baudelocque by means of calipers, and then making an average deduction, ascertaining the true conjugate and diagonal conjugate by means of the hand, fingers, or pelvimeters, are the most important. From the first little reliable information is to be gained in the majority of slight cases, and internal pelvimetry by means of instruments is manifestly so dangerous and uncertain a mode of procedure, that practically we are forced to have recourse to the hand.

By means of the extended hand, or closed fist, introduced into the passages, a pretty accurate and valuable estimate can be formed of the size of the true conjugate; but this, for obvious reasons, is almost solely available during labour, when the brim is not occupied by the fœtal head, or immediately after labour; its application in the non-pregnant state, or during the course of a pregnancy, would be accompanied by many difficulties, if not absolutely dangerous.

By ascertaining the dimensions of the diagonal conjugate with the finger, we can obtain reliable data from which to estimate the true conjugate. This method is perhaps the simplest and most generally applicable which has hitherto been at the disposal of accoucheurs, and can be made use of at any time in the non-pregnant state, during pregnancy, during labour so long as the head is situated above the brim, and after delivery. Where much contraction exists it is easy of application, and the information to be gained from it is of great value; on the other hand, however, when the contraction is slight, so much straining effort on the part of the examiner is necessary to push the soft parts upwards and aside, in order to reach the promontory of the sacrum, as to greatly diminish its value in practice. During and after labour, when the hand can be introduced with comparative impunity, the direct method is to be preferred to this one.

From these observations as to the means already at our disposal, it will be seen that in a certain class of cases—viz., those in which there is a minor degree of contraction in women who have borne no children, and in whom therefore direct internal pelvimetry could not with propriety be used—a more accurate method is desiderated; and it is with the notion that what is here advanced is a step in that direction that it is now brought forward. The method about to be described may be said to consist in directly ascertaining the dimensions of the conjugata vera by external manipulations, thus differing from all other modes. It is, in fact, measuring the conjugate plus the compressed anterior abdominal wall.

The examination is proceeded with as follows:—The bowels and bladder having been previously emptied of their contents, the patient is placed in the ordinary position for conducting an external examination of the pelvic viscera, on her back, with the thighs flexed on the abdomen. A point about an inch below the umbilicus, and in a line with it, is selected, over which the right hand is placed, and the anterior abdominal wall adpressed to the spinal column. By means of this manœuvre the situation of the promontory of the sacrum, which it is to be kept in mind has no immediate superincumbent soft parts, is readily ascertained. When the position of the promontory has been accurately made out, one extremity of an ordinary measuring tape is conducted to it, and kept firmly in contact with it by means of the thumb of the left hand, while with the right the tape is brought downwards and forwards to the upper and inner border of the symphysis pubis. In favourable cases this is all that is necessary, the inclination of the pelvis and the absence of soft parts immediately over the promontory of the sacrum rendering these manipulations less difficult than would at first sight be supposed. When the anterior abdominal flap is loaded with adipose tissue, it is frequently necessary to have an assistant, who presses the abdominal wall into the cavity of pelvis, which greatly facilitates accurate observations being made. To the measurement thus obtained has to be added the thickness of the firmly-compressed anterior abdominal wall; this I have found to be about 1-4th of an inch in healthy and ordinary females of child-bearing age.

The most favourable cases for the application of this method are those in which the abdominal wall is thin and lax; the most unfavourable are those in which it is loaded with fat and tense. In cases where there is much tension of the parietes the exhibition of an anæsthetic is of service. The advantages I would claim for it are:—

1. The ease with which it can be applied.
2. The reliability of the information to be gained from it.
3. The readiness with which it is submitted to by the patient.

The method described has marked limits of applicability. When the others fail it is most serviceable, and *vice versâ*. It is precisely in those cases where the internal methods of calculating or ascertaining the true conjugate cannot be applied with advantage that this one becomes available. It is inapplicable—

1. In advanced pregnancy.
2. During labour at or near full time.

It is of value—

1. In giving advice before marriage.
2. During the early months of pregnancy.
3. After labour as to the course to be pursued in subsequent deliveries, especially after first cases.

These remarks are based on twenty cases examined and measured during life, in which the accuracy of the determination was verified by inspection after death.

ARTICLE III.—*Case of Infantile Convulsions treated by Means of Bromide of Potassium.* By Surgeon-Major JAMES IRVING, M.D., Civil Surgeon of Allahabad, Bengal Presidency.

ON the forenoon of the 25th February I was called to see A. A., a very healthy robust boy of sixteen months old, the child of a member of the Civil Service. The little patient, I was told, had never suffered in any way since his birth, except occasionally from slight attacks of diarrhœa before some of the teeth came through the gums. He was now cutting his lower eye-teeth, having got the two upper ones about a month before; and though he had appeared all right when he got up in the morning, he had suddenly got convulsed just before I was sent for. As I was living about three hundred yards from the house where the child was, I saw him a few minutes after the attack commenced. As it was passing off, I lanced the gums freely, gave a warm bath, and as the bowels were loose and the legs drawn up as if there was griping, I ordered three grains of compound chalk powder, with half a grain of Dover's powder. The child seemed much relieved after this, and went off to sleep about noon. Cold was ordered to be kept applied to the head. About half-past two o'clock in the afternoon he again had convulsions, when I ordered twenty grains of bromide of potassium in an ounce of water, of which a teaspoonful was to be given every two hours; cold to the head was also continued, as it was slightly hot. The first dose of the bromide solution was given at 4.15, but the child was again convulsed rather violently at five o'clock, and his mother gave him another hot bath. I saw him for the third time at seven o'clock, when twitchings of the muscles of the arms had reappeared, the thumbs were turned in on the palms, and the poor little fellow seemed restless and very uneasy. In a short time the convulsive movements became very severe, and were first chiefly observable on the left side of the body, the left leg and arm being raised by sudden jerks. After a time both sides were implicated, and the convulsions became more violent, and continued without intermission till half-past ten o'clock. About eight o'clock, another warm bath was tried, and he had a teaspoonful of the solution of bromide of potassium; but he seemed to become worse, and the breathing became laboured and gasping, with long intervals between each breath. I thought that death was near. More for the purpose of appearing to the patient's parents to be doing something for the relief of the little sufferer than with any well-grounded expectation of benefiting him, the symptoms seeming so severe, and the remedy having as yet produced no apparent good effect, I poured all that remained of the bromide solution down his throat about half-past nine o'clock. Very shortly after this there was a marked change for the better; the convulsive twitches became less frequent and less severe, and eventually the child became calm, his breathing easier; he seemed inclined for sleep, and in an hour afterwards all convulsive movements had ceased, and he went off to sleep. The same solution was given in teaspoon doses at 11.35 of the 25th, and at 3, 4.35, and 9 o'clock of the morning of the 26th February. He had no more convulsions, and slept nearly the whole night.

I left him at 6 A.M. of the 26th, and saw him again about ten o'clock, when he seemed very languid and exhausted and still very drowsy, but he had taken a little milk-and-water during the night and again in the morning. The bowels were rather loose, and so continued for some days, and it was nearly ten days before he could run about as he had done before this attack. Altogether, between a quarter-past four o'clock P.M. of the 25th, when he had the first dose, till nine o'clock A.M. of the 26th, when the bromide solution was left off, he had taken close on forty grains.

Curiously enough, the morning before I was called to this case I had been reading in the January number of the *Practitioner* Professor Binz's article on the Therapeutic Employment of Bromide of Potassium, in which great doubts are expressed as to the supposed utility of bromide of potassium; but as my own experience did not lead me to agree with the Professor in his estimate of this drug, it struck me at once to try it. I may be wrong, but it seemed to me that the child derived much benefit from the remedy, which appeared effectually to check the violent convulsions from which he suffered, and I may say that I have seldom if ever seen a case in which continuous convulsions ever lasted so long. I should be inclined to try the same remedy in any similar case occurring in my practice. For all I know, this remedy in the convulsions of infancy may often before have been recommended, though I do not recollect noting it in any book or periodical.

ARTICLE IV.—*Valedictory Address to the Medico-Chirurgical Society of Edinburgh, delivered on the occasion of retiring from the Chair of the Society, 7th January* 1874; by P. D. HANDYSIDE, M.D., F.R.S.E., formerly Senior President of the Royal Medical Society and Senior Acting Surgeon to the Royal Infirmary; Teacher of Anatomy in the Edinburgh School of Medicine. Part IV.

(Continued from page 1020.)

XII. MEDICAL JURISPRUDENCE.

In 1822, *Dr Charles W. Coindet of Geneva* related a "Case,[1] accompanied with General Reflections on the Action of Poisons." In 1823, *Dr C. W. Coindet* read "On the Action of Oxalic Acid as a Poison." *Drs Christison and Coindet* read "Inquiry into the Action of Oxalic Acid as a Poison, Part I.;" *also*, in the following month, "Part II." In 1825, *Mr Gavin Milroy*, "Case in which a Considerable Quantity of Sugar of Lead was Swallowed." In 1826, *Dr Christison* read "An Account of Several Cases of Poisoning with Arsenic, in Illustration of the Delicacy of the Chemical Evidence, and the Force of the Evidence derived from Symptoms." In 1827, *Dr Combe of Leith*, "On the Poisonous Effects of the Mussel (*Mytilus edulis*)." In 1829, *Dr Gairdner*, "Account of the Deleterious Effects of Coal-Smoke on a Family consisting of Six Adult Individuals." In 1832, *Mr A. Watson*, "Three Cases of Homicide, in which Insanity was pleaded in Exculpation—Wounded Parts Exhibited." In 1833, *Mr A. Watson*, "Cases of Ruptured Intestine from Contusions;" *Dr M'Whirter*, "Case of Unusually Long Suspension of Animation in a New-born Infant;" and *Mr Watson*, "Medico-Legal Cases of Drowning, with Remarks." In 1835, *Dr Christison*, "Case of Fatal Poisoning with Muriate of Morphia;" and *Mr George Glover*, "Case of Recovery from Suspended Animation." In 1836, *Dr Martin, Leadhills*, "Letter on the Poisonous Effects of Lead." In 1837, *Dr P. D. Handyside*, "Account of a Case of Suicide from dividing the Internal Jugular Vein, and the *sudden* introduction of Air in *large* quantity into the Heart,—with Preparations." In 1838, *Dr P. D. Handyside*, "Case of Suicide in a Female from the Introduction of a Firm Plug into the Fauces,—with Preparations;" *Mr Watson*, "Case of Suicide by Sulphuric Acid, and Wounded Veins of Neck, with Air into the Vascular System." In 1840, *Dr T. T. Crawford, Tynemouth*, "Case of Poisoning by Muriatic Acid." In 1843, *Dr Christison* read "An Account of some Obscure Cases of Poisoning;" *Dr Cormack*, "Cases of Transient Insanity, with Remarks upon their Importance in Forensic Medicine;" and *Dr Jackson, Leith*, "Remarkable Case of Suicide by the Introduction of a Large Key into the Œsophagus, with Remarks—Preparation shown." In 1844, *Dr Christison*, "On

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[1] See April No., page 906, under "Therapeutics."

Poisoning by Sulphate of Iron;" *Dr Jackson, Leith,* "Case of Death from a Piece of Potato-skin in the Larynx;" and *Dr James Andrew,* "Case of Poisoning with Corrosive Sublimate." In 1845, *Dr Bennett,* "Case of Poisoning with Hemlock, the Facts agreeing with the Account of Plato and Meander of the Effects produced by the Κωνειον or State Poison of the Athenians." In 1847, *Dr Thomas Anderson,* "Case of Recovery from a Poisonous Dose of Strychnine, with Observations on the Tests of the Organic Alkalies." In 1851, *Dr Andrew,* "Case of Poisoning by Atropine." In 1852, *Dr Seller,* "On the Indications of Apoplectic Death in Persons Found Dead,—with a Case;" and *Dr Alex. Wood,* "Case of Poisoning by Oxalic Acid." In 1855, *Dr Keiller,* "Observations on Manual Strangulation." In 1856, *Dr Christison,* "Observations on the Trial of Mr Wooler for Poisoning by Arsenic." In 1857, *Mr G. W. Spence, Lerwick,* "Two Cases of Hæmorrhage from Contused Wounds of the Female Genitals, with Remarks on the Diagnosis between Incised and certain Contused Wounds." In 1858, *Dr J. Ivor Murray, China,* "Note of the Result of an Analysis of a Portion of the Bread with which A-lum was accused of Poisoning the European Residents at Hong-Kong." In 1859, *Dr Andrew Myrtle, Polmont,* "Case of Criminal Abortion at Polmont, with Comments on the Medical Evidence." In 1862, *Dr Yellowlees, Morningside,* "Homicidal Mania, a Biography, with Medico-Legal and Physiological Comments." In 1865, *Dr Cairns* "showed a Specimen of Poisonous American Cheese which had produced Alarming Symptoms in at least Twelve Cases." In 1867, *Dr Manners, Jamaica,* "Notes of Cases of Poisoning by means of Susumber Berries, with Observations by *Dr John Millar, F.R.C.P.E.*" In 1868, *Dr Sanders,* "Case of Poisoning with the Rhus Toxicodendron." In 1870, *Dr Bennett,* "On Chloral as the Antidote to Strychnine—Illustrated by Experiments on Rabbits." In 1871, *Drs R. Turner* and *R. S. Turner, Keith,* "Cases of Poisoning by Carbonic Acid Gas, with Remarks." In 1873, *Dr Angus Macdonald,* "Note of a Case of Delirium Tremens, complicated with Atropia Poisoning."

At the Conversazione, *Dr Douglas Maclagan* exhibited "Two forms of Dialysing Apparatus, as used in Medico-Legal Analyses for separating Soluble Crystalline Poisons from Organic Fluids, such as the Contents of the Stomach. For such purposes the method leaves nothing to be desired, rendering unnecessary, as it does, the tedious and troublesome processes of precipitation and filtration required to separate the animal matter, and obtain a clear fluid to which the appropriate tests may be applied. The dialysing medium used in the apparatus exhibited is the well-known substance, vegetable parchment, which is so arranged that the suspected matter is placed on the inside of the membrane, and distilled water externally. Diffusion of the fluids takes place, and the crystalline poison, if present, passes into the distilled water, leaving the colloidal matter behind, and thus an almost perfectly clear solution is obtained, which may be readily tested. Our apparatus was exhibited *in action*. A little milk, to which there had been added a small quantity of oxalic acid, was placed in the apparatus at 2 P.M., and by 8 P.M. the presence of the poison in the surrounding distilled water was demonstrated by the ordinary test of sulphate of calcium. In cases where a crystalline poison is present in some quantity, evaporation of the clear dialysate admits of the poison being obtained in substance. A specimen of crystals of salt of sorrel (binoxalate of potassium), so obtained from the contents of the Stomach in a Fatal Case of Poisoning, was exhibited."

Dr Littlejohn exhibited "Illustrations of the Science of Medical Jurisprudence, in the shape of Imprints of Footmarks in the Snow, in Ploughed Land, etc., and the Appearances in the Body after Poisoning."

Mr Andrew Davidson, Medical Missionary, Madagascar, showed "The Tangèna (*Tanghinia venenifera*), or Ordeal Poison of Madagascar. The poisonous part is the oily seed within the kernel, which owes its properties to an alkaloid named *Tanghinia*. It was administered in an emulsion made with the juice of the banana. When it acted promptly as an emetic, the person who had taken it survived, and was considered innocent of the crime of which he had been accused. If not speedily rejected, it acts as a purgative, destroys the conductivity of the motor nerves, causes a tingling of the parts to which it has been applied, but does not produce paralysis of sensation, proving fatal by arresting the action of the heart. When death resulted, this was regarded as conclusive evidence of the guilt of the accused."

XIII. STATISTICAL PAPERS.

If it be true that "Humanum est nescire et errare," this is specially so with regard to an inexact science like Medicine, as may be noticed in the many sources of difficulty and fallacy ever occurring in our diagnosis. No man is fertile in resource and sound in precept who has not had much experience; while the record and tabulation of the Experience—in other words, of the Facts—ascertained by a few careful observers is of inconceivable value in enriching the domain of Medicine. As *Abercrombie* has said,—"The great agent by which we—both as intellectual and moral beings—are acted upon, is Truth." Thus, it was from careful study and observation of hard facts regarding the issue of Operations for Cancer, that our *Liston* considered that "the circumstances *must be very favourable indeed to* induce a surgeon to recommend, or *warrant him in* undertaking, an operation for the *removal of malignant disease;*"[1] that our late distinguished member, and former President, *Mr Syme*, went so far as to state his "*conviction* that it would be better, *both for the interests of humanity and the*

[1] As quoted in *Dublin Medical Press*, April 9, 1862.

credit of surgery, if the operation were entirely abandoned;"[1] and that *Sir Benjamin Brodie* asserted that, "in the *larger* proportion of cases in which the operation is performed, the patient is *not alive* in two or three years afterwards."[1] During the last half-century, we have accumulated stores of honest and careful Statistics of much value, if we could simply induce some of our number to arrange and apply them. Thus, our late loved *Alison* evinced his love of Fact in *the first* paper that he proffered to the Society, and that so early as 5th December 1821. His was a mind well fitted to weigh evidence in matters of such special difficulty to deal with as Statistics confessedly are; where, as it has been said, "so many considerations interpose, that a *post hoc* cannot become a *propter hoc* argument in matters involving so many qualifying circumstances." *Dr Alison's* paper was entitled, "Two Tables; to be continued Quarterly: the one containing a list of the Diseases of the Patients admitted at the New Town Dispensary during the last quarter; the other, a List of the Deaths in the Practice of that Institution, with the Ages and Diseases of the Dead: accompanied with a short Statement of the purposes to be served by such Registers, if accurately kept for some time together."

Again, in the second year of our Society's existence, we issued Schedules to all the parish clergy throughout Scotland, soliciting Statistical Returns, in a tabular form, relative to the Health, Diseases, and Mortality of the people throughout Scotland. During the same year, *Dr Thomas Anderson, Inspector of Health of Shipping, Port of Spain, Trinidad,* presented to the Society a paper on the "Practical Results in the Treatment of Tetanic Diseases collected in the course of Five Years;" and *Dr Alison* communicated his "Observations, with Tables, on the Probabilities of Life."[2] In 1824, *Dr Webster of London* furnished "Tables of Deaths in the Practice of the London Infirmary for Sick Children for three years;" and *Dr Alison* gave a "List of Diseases admitted into the Montreal General Hospital in 1822–23." In 1830, *Dr Abercrombie* issued his felicitous "Circular as to a Collection and extensive Comparison of individual Facts in aid of the Promotion of Medical Science." In 1835, *Dr John A. Robertson* furnished data in a Tabular form, "showing the relative success attending the operations of Extraction and Displacement of the Cataract." In 1836, *Dr Knox,* "Contributions to the Statistics of Hernia;" *Dr J. Smith,* "Contributions to the Statistics of Lunacy;" and "*Dr Smith* was requested to correspond, in the name of the Society, with the Medical Officers of the Public Lunatic Asylums throughout Scotland, and to endeavour to obtain from them information relative to those points connected with the Statistics of Lunacy to which his own Communication refers." In 1839, *Dr J. Y. Simpson* wrote "on the Alleged Infecundity of

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[1] See previous note.

[2] See *Med. Chir. Trans.*, vol. i. pp. 384-395.

Free Martin Females;" *Dr John Reid*, "Results of the Post-mortem Examinations of Fever Patients made in the Royal Infirmary during the last twelve months;" and *Dr J. Smith* gave "General Results of Fatal Cases of Insanity formerly submitted to the Society." In 1840, *The Medical Officers of the New Town Dispensary*, "Statement of the Diseases observed in the Practice of that Institution during 1840;" *also*, in 1842, "Statistical Report for the year 1841, communicated by *Dr Omond*;" and *Dr Smith* gave "Tables showing the result of 683 Cases of Insanity treated in the City Asylum." In 1843, *Dr P. S. K. Newbigging*, "Report of Cases treated in the Edinburgh Lock Hospital, with Observations;" *Dr Maclagan*, in a Valedictory Discourse, addressed the Society upon the great value of Statistics; *Dr Omond* gave a "Statistical Report of the New Town Dispensary for the year 1842;" *Dr Graham Weir*, "A Report of the Obstetrical Department of that Institution for the last three years;" and *Mr Benjamin Hobson, M.B.*, "Report for 1841–42 of the Medical-Missionary Hospital at Macao, China." In 1845, *Drs A. D. Campbell* and *Halliday Douglas*, "Statistical Report of the New Town Dispensary for 1844." In 1846, *Dr Tait*, "Report of the Sickness of the Edinburgh Police Force for 1845, as compared with that of the Yearly Friendly Societies;" *Dr J. Y. Simpson*, "On the Duration of Labour as a Cause and Criterion of the amount of Danger and Fatality to the Mother and Infant: A Statistical Inquiry, founded upon Dr Collins' Dublin Hospital Reports;" *Dr Begbie*, "Observations on the Mortality of the Scottish Widows' Fund Life-Assurance Society, from 1815 till 1845;" and *Dr A. H. Douglas*, "Analysis of the Statistical Returns of the Edinburgh and Glasgow Infirmaries for the year 1845." In 1847, *Dr Tait*, "Annual Report of the Diseases of the Edinburgh Police Force;" and *Dr J. Y. Simpson*, "Report of St John Street Maternity Hospital." In 1848, *Dr Skae*, "Report of the more remarkable Cases treated in the Edinburgh Royal Asylum in 1847, with Remarks." In 1851, *Dr Robert Hamilton* and *Mr Bell*, "Medical Report of the Edinburgh Eye Infirmary for 1850." In 1852, *Dr W. T. Gairdner*, "Notice of Post-mortem Examinations in the Royal Infirmary of Edinburgh, 1851–52, illustrated by Preparations and Drawings." In 1853, *Dr Gillespie*, "Analysis of the Cases of Scarlatina that occurred in Donaldson's Hospital during Autumn 1852, with Remarks on the State of our Knowledge of that Disease;" *Dr Christison*, "Investigation of the Deaths among the Assured in the Standard Assurance Company;" and *Dr Begbie*, "On the Causes of Death among the Assured in the Scottish Widows' Fund and Life-Assurance Society, from 1846 to 1852." In 1856, *Dr P. M. Mess, Holland*, "Report of the Sea-Bathing Season in Scheveningen of the Years 1854 and 1855." In 1857, *Dr George Playfair*, "On the Prevalence of Calculus Vesicæ and the Results of Lithotomy in the Bombay Presidency;" *Dr Christison*, "Second Quinquennial Report on the

Deaths in the Standard Assurance Company for the period ending 15th December 1857." In 1860, *Dr Begbie*, "Report on the Causes of Death of the Scottish Widows' Fund Life-Assurance Society from 1853 to 1860; with Observations, Medical and Statistical." In 1862, *The Medico-Chirurgical Society* prepared a "Scheme for the Periodical recording of Facts relating to the Prevalence of Epidemic and Acute Disease within the experience of Members." In 1864, *Dr M. Duncan*, "The Relations of the Weight and Length of the Newly-born Mature Child to its Mother's Age." In 1865, "*Dr Haldane* referred to some Royal Infirmary Cases of Pneumonia as illustrating the Fallacy of the Indiscriminate Application of the Statistical Method to the Determination of the Effects of Different Modes of Treatment;" and *Dr Tuke*, "On the Statistics of Puerperal Insanity." In 1869, *Dr Joseph Bell*, "A Contribution to the Statistics of Femoral Hernia." In 1870, *Mr Spence*, "Remarks on the Results of the Greater Amputations, illustrated by Tabular Views." In 1873, *Mr A. Davidson, Medical Missionary, Madagascar*, "Lithotomy Statistics at the Antananarivo Hospital;" and *Dr Joseph Bell*, "Surgical Cases, with some Notes on Temperature after Operations, and Tabular Illustrations."

XIV. PHILOSOPHICAL PAPERS.

We have notices upon our Minutes of papers of merit that have come from members of a philosophic and often of a dispassionate habit of mind, men of erudition, possessed of a body of general knowledge, and at the same time conversant with medical science. In 1823, *Dr Robert Hamilton* read a communication "On the Fever that occurred in the Magdalene Asylum in 1821, as illustrating the Influence of Panic in propagating Contagious Diseases." The late *Dr John Scott*, an old Army Surgeon, and latterly Physician to the Queen for Scotland, read, in 1834, "Case of a peculiar Mental Affection depending upon Tænia." In 1835, *he* read a series of Cases illustrating the "Effects of Mental Emotion in the Production of Disease." In 1836, a paper on the "Structure of the Biliary Ducts as connected with Jaundice produced by Moral Causes," was read by *Dr Knox*. In 1837, *Dr W. Thomson* read, "On the Sources of Difficulty and Fallacy in Diagnosis." *Dr Seller*, in 1843, read two papers, "Examination of the Plea of Insanity against the charge of Murder," Parts I. and II. In 1848, *he* read "On the Signification of Fact in Medicine, and on the Hurtful Effects of the *incautious* use of such modern sources of Fact as the Microscope, the Stethoscope, Chemical Analysis, Statistics," etc. In 1851, *Dr Alexander Wood* read, "Contributions towards the Study of certain Phenomena which have recently been denominated 'Experiments in Electro-Biology.'" In 1854, *Dr J. Matthews Duncan* read a paper "On the Statics of Pregnancy." In 1855, *Dr Alexander Wood* communicated a paper on "The

Nervous Element in Inflammation, and its Influence on Treatment." In 1858, *Dr Skae* read one "On Moral Insanity and Oinomania." In 1868, *Dr Arthur Mitchell, Commissioner in Lunacy,* read a paper with the title, "The Bodily and Mental State of the Pregnant and Nursing Mother, in connexion with Idiocy in the Offspring." In the same year, *Dr Grainger Stewart* wrote on "Gangrene from Nerve Influence;" and *Dr Argyll Robertson* on "Sympathetic Retinitis Pigmentosa." Lastly, in 1873, *Dr Ea*[illegible] *of Darlington,* contributed a paper on "Darwinism in its relation to the Higher Faculties of Man."

XV. LITERARY PAPERS.

Our Medical Literature has, in no mean degree, been enriched by the mental activities of our members—chiefly, however, I regret in having to add, our *earlier* members—who shed honour on their profession, having been equally distinguished by their professional eminence and their general culture; in this resembling Galen himself, who is eulogized by Dr Friend, in his "History of Medicine," as having been not only the *best* Physician, but the *best* Scholar and Writer of his time. While it is admitted that previously unrecorded Cases of interest serve, as Abercrombie in his Presidential Address indicated, as a valuable contribution to Medical Literature, a broad view of these, in a summary fashion, including cautious generalizations from this aggregate, forms the only accessible mode of drawing fair Deductions from well-ascertained Facts. Much might be done by Members in the bibliographical department of work, by recovering old and often much-forgotten medical literature relative to the history of extinct Superstitions, or of certain lines of Practice or of Operative procedure, as well as the recovery of lapsed views that once prevailed in regard to the Nature and Seat of certain diseases.

As matter of fact, however, this Society has occasionally delegated to Committees of their number, the duty of obtaining from Authors of Papers already read, and from other sources, additional facts relative to topics brought before the Society; and these Committees have sometimes sat "*de die in diem,*" till the wished-for details were expiscated. Of these very interesting Reports we have instances on the subjects of Cholera, Ovariotomy, Chloroform, Fatal Hæmorrhage, Epidemic and Acute Diseases, and on special parts of the Anatomy of the Brain.

Three octavo volumes of "Transactions," containing many of the earlier papers read before the Society, were published in the years 1824, '26, and '29, and are well known to the Profession. At a later period the "Transactions" occupied a prominent place among the original matter in the *Edinburgh Medical and Surgical Journal;* to which valuable periodical—started at the same time and place with the *Edinburgh Review*—many a fasciculus of its papers were contributed through a Committee of our Society. Subsequently, the "Proceedings" of the Society have been recorded in the *Edinburgh Medical Journal;* where, also, many of the Papers are printed in full. Further, the three English and the one Irish weekly journals have, for some years, contained short Notices of our "Proceedings."

In 1823, *Dr James Molleson* read "A Sketch of some Modes of Practice pursued in Diseases obstructing different of the Natural Passages." In 1824, *Mr W. Hamilton, H.E.I.C.S.*, read "Observations on the Nature, Causes, and Treatment of Beri-beri." In 1825, *Dr John Wilson of Hull* communicated "Observations on Syphilis, chiefly founded upon its History at Otaheite." In 1827, *Dr Alison* read "Cases of Disease of the Brain and Nerves, illustrating certain recent Doctrines." In 1828, *Dr W. H. Ruan of St Croix, W.I.* read an "Account of a Contagious Epidemic Disease (Break-bone Fever), which made its appearance in the Island of St Croix, in October 1827;" *Dr George W. Stedman* read an "Account of an Anomalous Disease which raged in the Islands of St Thomas and St Croix during the Autumn and Winter 1827–28;" and *Assistant-Surgeon Walsh, 89th Regiment,* read a "Narrative of the Diseases under which the British Troops suffered during the Burman War." In 1832, *Mr John W. Turner* read "Report of the Society's Committee upon the Statements published by Mr John Lizars on the subject of his Paper on the Pathology of Cholera." In 1835, *Dr W. Thomson* gave an "Abstract of Cases in which a portion of the Intestinal Canal has been Discharged by Stool, with Remarks;" and *Dr D. Craigie,* "Cases of Perforation of the Stomach." In 1836, *Dr Craigie* read an "Abstract of Cases in which Pseudo-Membranous Substances have been Discharged by the Bowels;" *Mr W. Wood,* "An Account of Scarlet Fever as it appeared in Heriot's Hospital, and other Public Institutions in Edinburgh, during the present Epidemic;" and *Mr Chas. Sidey,* "Notices of the Epidemic Scarlatina as it has prevailed in Edinburgh since the Autumn of 1835." In 1841, *Dr J. Y. Simpson* "Antiquarian Notices of the former Existence of Leprosy and Lazar-Houses in Scotland." In 1842, he gave "Notices regarding the first appearance of Syphilis in Scotland;" and *Dr Mackinnon* a "Medical Report of the Royal Lunatic Asylum, Morningside." In 1843, *Mr Harry D. S. Goodsir* read "An Account of a Form of Continued Fever, accompanied by Jaundice, which occurred in the East of Fife in 1841–42; with some Observations on the Gastro-Intestinal Character of the Endemic Fever of that District." In 1845, *Dr Mackinnon* read his "Report of the Royal Lunatic Asylum for 1844." In 1846, *Dr Parker, Canton,* "Notes of Surgical Practice amongst the Chinese;" and *Dr Mackinnon,* "Report of the Royal Lunatic Asylum for 1845." In 1847, *Dr J. Y. Simpson* read "Historical Notes regarding the Superinduction of Insensibility to Pain in Surgical Operations;" and *Dr Bennett, Reporter,* "Report of the Committee appointed by the Society to Investigate the Properties of Chloroform." In 1848, *Dr John Brown*, "Notice of the Times, Life, Character, Philosophy, and Practice of Sydenham, with Remarks upon the History of the Art of Healing, as comprehending a Science and an Art, and on the Specific Difference between Science and Art—between Definition and Description, with a few Illustrative Cases." In 1853, *Mr Syme*, "On the Improvements which have been Introduced into the Practice of Surgery within the last Thirty Years;" *Mr Miller*, "Observations on the recent Progress of Surgery;" and *The Society* presented "Petitions to both Houses of Parliament in regard to Medical Reform." In 1855, *Dr W. T. Gairdner*, "Account of certain Anomalous Cases resembling Variola and Scarlatina;" we have a "*Report of the Committee of the Society* on Mr Miller's Case of Fatal Hæmorrhage;" and *The Society* presented "Petitions to both Houses of Parliament on behalf of the Naval Assistant-Surgeons." In 1856, *Dr Priestley* read "Report of the Society's Committee upon the Preparation in Dr William Brown's (Melrose) Case of Fatal Hæmorrhage into the Abdomen connected with Disease of the Ovary;" and *Dr F. Campbell Stewart, New York*, "On the Medical Schools, and the Condition of the Medical Profession, in the United States of America." In 1857, *Dr Charles Wilson* read "Dionisio Daça Chacon: A Medico-Historical Sketch;" *Mr Alex. Fiddes*, "Observations on the Tubercular and Anæsthetic Leprosy as they occur in Jamaica—Illustrated by Casts and Drawings;" and *Dr Matthews Duncan*, "Some Notes on the History of the Decidua—William and John Hunter." In 1858, *Dr John Brown*, "Dr Andrew Brown of Dolphinton, and his Times." In 1860, *Dr Alex. Wood*, "Smallpox in Scotland, as it Is, Was, and Should be: some Suggestions for a Vaccination Bill." In 1861, *Dr Charles Wilson*, "Obituary Notice of the late Andreas Retzius of Stockholm;" *Mr Alex. M. Edwards*, "Note on the History of the Radical Cures for Hernia;" and *Dr J. W. Begbie*, "On Ichthyosis—its Particular Forms; Historical Account of the First Description of the Disease, etc. In 1862, *The Society* issued to its Members a Tabular "Scheme for the Periodical Recording of Facts relating to the Prevalence of Epidemic and Acute Disease, with a view to the Results being brought before the Society at its Ordinary Meetings, and in the form of Quarterly or Half-yearly Reports;" "Report by the Council on Epidemic Diseases till end of April;" "Report by the Council on Epidemic Diseases till end of May;" and *Dr Clouston, Morningside*, "On the Connexion between Tuberculosis and Insanity." In 1863, *Mr P. K. Vartan, Medical Missionary at Nazareth*, "On Arabian Lithotomy, with a Description of the barbarous methods in use among the Natives." In 1864, *Dr Andrew Wood* read a Paper "On Medical Education;" *Mr Andrew Davidson, Madagascar*, "An Account of Tubercular Leprosy in the Island of Madagascar;" and *Dr Craig Maclagan*, "On the

Arsenic-Eaters of Styria." In 1867, *Mr Andrew Davidson*, "An Historical Sketch of Choreomania, with some Account of the Disease as recently observed in Madagascar." In 1873, *Mr Andrew Davidson*, "Brief Retrospect of the recent History of Lithotomy, with an Account of an Improved Method of performing the Operation."

At the Conversazione, *The Royal College of Physicians* had the courtesy to permit their valuable Collection of Portraits of celebrated Physicians and Surgeons, and many rare and valuable works in Medical Literature, to be exhibited. *Dr Charles Bell* showed the original Etchings, Drawings, and Manuscripts of his uncles, Mr John Bell and Sir Charles Bell. *Dr George Balfour* showed "*Cruveilhier's* Anatomie Pathologique, folio; plates of Corneous Formations." *Dr Alexander Simpson* exhibited a Chinese work in three volumes on the Arts and Sciences, with Illuminations, and a volume of Drawings of the Small Bronzes found in the Excavations at Pompeii. *Mr Andrew Davidson, Madagascar,* exhibited some interesting Malagasy Medical Literature. *Dr P. D. Handyside* showed an Illustrated Copy of Theophrastus; Anatomical Works in the Arabic, Tamil, and Chinese Languages, by *Benjamin Hobson, M.B., London,* Shanghae Mission Press, folio, 1858; by *Dr John Wortabet, Beyrout,* 1872; and by *Dr S. F. Green, Jaffna,* Ceylon Mission Press, 1872;—together with recent Chinese volumes in folio on Physics, Chemistry, Botany, Zoology, Physiology, Materia Medica, Surgery, and Medicine, by *Mr Benjamin Hobson,*—many of them curious, and all well illustrated.

XVI. GENERAL POSITION OF THE SOCIETY.

First,—Its Duty to Science.

While it is admitted, in the words of Dr John Young, that "no profession has contributed *so much* to general science as that of medicine," and that the leisure possessed by the younger members of our profession "ought therefore to be fruitful of solid contributions to the sciences," the Medico-Chirurgical Society has been shown, I hope, to have done no inglorious work in assisting to raise the Profession of Medicine to the honour and dignity of a Science; although it may not yet have fully attained that eminence. Permit me, then, for the nonce, as with the voice of Abercrombie, or of Davy, of Kellie, or of Simpson,—who as young members were, in the love they bore to Science, hard-working ones, and whose labours *totis viribus* we all respect,—in presence of such an incentive, to invite our younger members especially, by methodical business *arrangements* of life, and by strenuous efforts of *mental* activity, to put forth more of their latent energy, and so to reinforce and help forward our Profession, that it shall come to be fully recognised as an active force in Science properly so called; and they assuredly will find its "value and rank to be," as Sir Joshua Reynolds said of Art, "in proportion the *mental labour* employed in it, or the *mental pleasure* produ by it."

I may here just allude to the indefatigable industry of our Edinburgh Bar, the members generally of which have earned the hi reputation of being accomplished men in Literature, Philosop and Science; having become so, in a great measure, at the out of their career; *through* storing their minds by hard reading, at a time long anterior to their becoming fully engrossed in the ardu labours of their profession.

Amid the absorbing pursuit of even General Practice, in which every man has a tendency—it may be insensibly—towards a *spécialité*, let the cultivation of it tend towards making Medicine more of a Science. Sir David Brewster wrote, in 1828, these golden words of counsel to Principal Forbes, then studying law: "I trust you will not allow any professional pursuits to interrupt your studies and researches. The cultivation of Science is a luxury of no common kind amid the bustle and vexation of life, and is quite compatible with the most active professional duties." Let us bear in remembrance these noble words of our ordinary member's Diploma, "*Virum . . . . quem scientiam medicam* ingenio felici et laudabili diligentiâ coluisse *compertum habemus, in numerum Sociorum adscripsimus.*" "Who excuses himself," the French proverb says, "accuses himself," and thus deprives us of the high gratification of witnessing in this Hall the development and the training of rising talent and professional eminence. If, with Mr P. G. Tait, "the basis of Chemistry is the indestructibility of matter, and that of the rest of Physics is the indestructibility of energy," let me ask, Why should not the basis of the Medico-Chirurgical Society continue to be recognised in all time coming as THE INDESTRUCTIBILITY OF PROGRESS, till our noble profession reaches somewhere beyond its present stage of infancy in Science?

Secondly,—ITS ORDER OF BUSINESS.

The objects and characteristic features of our Society have undergone since its foundation no change; and, time-honoured, these testify to the sagacity and far-sightedness of its Founders. The main elements of our coherence and stability seem to consist (1) in the Catholicity of our professional Membership; (2) in the mutual confidence resulting therefrom; and (3) in the interest taken by our intelligent membership in the Papers and Communications tabled on every branch of Medical Education no less than in every department of the Healing Art. Thus, a common weal, dependent one on another for its prosperity, flourishes amongst us. That this comprehensive basis of Fellowship is appreciated, is shown by the attendance at our meetings of Brethren who are in full employment in a Profession "perhaps the most capricious and susceptible of all professions," who yet readily sacrifice ease to duty, and *find* time to attend our meetings on the admitted principle that, *the more men have to do, the more time have they at command*—brethren who, with Faust's Famulus, might say, as regards the business of our Society, " Zwar weiss ich viel, doch möcht ich alles wissen," which, poorly rendered, may signify—*Although I know much, yet I would know everything.* It is matter of regret, however, that with 219 names of members on our roll, our audience is comparatively small; nevertheless, that select audience happily is "*fit* though few."

As regards Communications on Practical Subjects, our principle, as professional men, being, *first,* not to write or talk without a practical aim in view,—and, *secondly,* not to advance a single position which we have it not in our power to establish,—Members, from experience, know well for their encouragement the truth—" Fiat justitia, ruat cœlum "—*I am sure justice will be done to my* Paper, *I don't care what happens.* Reynolds once observed that it was impossible for two painters, in the same line of art, to live in friendship; but, trained in this Society, our usages raise the tone of debate; and thereby, to use one of Chalmers's fine phrases, " the breath of" our " Society" is " sweetened;" and although we would remark, with the famous Sir Roger de Coverley, that much may be said on both sides, yet, to give a favourite quotation from Burke, " Our antagonist is our helper. This amiable conflict with difficulty obliges us to an intimate acquaintance with our object, and compels us to consider it in all its relations."

During my occupancy of this Chair, it is gratifying to add, there has existed uniform harmony among us; and whilst it is a duty of your Chairman to protect Members in the right of free discussion, I have never seen their susceptibilities wounded in any manner, even by a Member in possession of the house being interrupted; nor have I once had occasion to keep order or maintain authority:—such have been the pleasant and thoroughly cordial relations which have uninterruptedly subsisted within these walls. Moreover, our discussions have been conducted in a highly courteous and suggestive manner, and in a judicious and significant spirit; much has been said and done to engender good feeling; while occasionally there has sparkled out a little mental sunshine as a pleasant transition in some rather long debate. Indeed there has been exemplified here,—in the leading Society of our Faculty in this our mother-city,—" in certia unitas, in dubiis libertas, in omnibus caritas."

Thirdly,—ITS TREASURERS, SECRETARIES, AND COUNCILLORS.

Although we cannot exhibit an edifice of stone and lime belonging to us, for we never had a monetary grant or legacy, yet our TREASURERS from the very first have been singularly prudent men, and, after having voted liberal sums towards several professional objects, including the new Medical Hospital, our Society's balance-sheet points to £200 in the bank. In allusion, however, to our humble means hitherto mutually contributed, and in estimating th services to medicine of our Association, it is readily admitted th while, like every Society planted on a philosophic basis, it h been specially animated in all its labours by the strictly scient method of Aristotle, yet I may be permitted the indulgence t add, that the Medico-Chirurgical Society of Edinburgh has a cla to rank under the banner of the peripatetic philosopher perhaps *too* literally for comfort, inasmuch as we have maintained, as a body, a very vagrant and migratory character during the first forty years of our existence. For this long period of time we ha indeed no fixed *habitat*. Resting on an average scarcely mor than two whole years in one place of meeting, we frequented n less than eighteen distinct and successive places of resort, having been driven about to hold concourse often elsewhere than, as it were, *inter sylvas Academi*. Thus, we have from time to time often communed in very strange purlieus, to say the least. Commencing in 1821 with a hotel in Register Street, we next met in one at Waterloo Place. We next were accommodated within the old and very beautiful building of the Royal College of Physicians, 12 George Street; we then moved to Nos. 5 and 7 Hill Street; afterwards to the Antiquarian Society's Committee Room in the Royal Institution; then in succession to 13 Queen Street, 119 George Street (Royal College of Physicians' temporary apartments), 6 York Place, 91 Princes Street (Mackay's Hotel), 19 Princes Street (Tait's Hotel), 65 George Street, 45 George Street (Cay & Black's Saloon), 51 George Street (Hall), 5 Queen Street, 98 George Street (Freemasons' Hall), 79 George Street (Smith & Philpot's Auction-room), 60 Princes Street (Photographic Saloon), until at length our Treasurer, in June 1862, engaged for us accommodation in No. 117 George Street, the very suitable hall within which we have since continued to hold our meetings.

Again,—we have had the good fortune to possess SECRETARIES with the necessary threefold qualification of (1) Professional accomplishments, (2) Courtesy in dealing with fellow-members, and (3) Business habits. They have been always at their post in preparing the Business to come before you; and in that important matter, as well as in exercising caution lest any Dogma, Dictum, or line of Practice should go forth of a kind to compromise this Society, they have met your approval. In all *other* matters, also, committed to them, they have striven to maintain intact the reputation of the Society, and have merited our warm thanks.

And again, to the COUNCILLORS, past and present—elected, like our other office-bearers, after the manner of other scientific bodies *deserving of public confidence*, namely, by the voting papers of *all* the members present—I beg now, in a representative capacity, to return cordial thanks for their uniform assistance and support.

XVII. CLOSE.

Having now, Gentlemen, taken, as it were, a *coup d'œil* of the Origin, Progress, and Prospects of our Society,—having endeavoured to show how we may each annex our mark upon Medical Science, —having attempted to impress on the Society some things of *practical* utility; feeling withal how inadequately I have treated the comprehensive subject upon which, according to the measure of my ability, I have dared to venture—"too briefly for its merits, but too prolixly for your patience"—it remains for me simply to thank you for your intelligent sympathy.

My apology for venturing upon this ground is its being both natural and becoming in one holding your *Jubilee* Presidency to take a Retrospective view of our Proceedings, as a wholesome stimulant to future exertion in this our day of immensely advancing Discovery; for we have the satisfaction of knowing that this Society, which from its onset has ascertained and recorded many priceless facts,

> Et magnâ testatur voce per umbras;
> Discite justitiam moniti, et non temnere Divos,
> (VIRG. *Æn.* vi. 618)

has at the present moment a character for possessing a band of Cultivators of Medical Science *second to* NONE;—whose intelligence, and acquirements, and zeal, do it honour;—and from whose prospective labours we may form some estimate of good service to be yet done to Scientific Medicine.

I have farther felt that I could confidently reckon on, what long experience has taught me, the generous indulgence of an assembly of my professional brethren, from whom I have met with nothing but kindness and consideration. One has said that "to enjoy the confidence of those amongst whom their lives were spent was one of the greatest pleasures given to man, and without it life would be worthless;" and sorry should I be to forfeit your good opinion, or afford you cause of regret that my name has so long stood on the roll of this world-known Society. May its career in the future be, at the least, as distinguished as it has been in the past:—*Esto perpetua!*

I now retire from this Chair, awarded to me unanimously in 1871 in the exercise of your prerogative. An influence *you* are, which I defer to *the more*, inasmuch as you are the *only* broad basis or organization existing, through which the United Profession in this City and School of Medicine is capable of administering its opinion, and of conveying in trust the highest professional honour that it has in its power to bestow.

But though "Le Roi est mort, VIVE LE ROI!"—Time has no interregnums; neither has the Medico-Chirurgical Society, the Chair of which I have the honour to vacate in favour of *the President-Elect, now* to be inaugurated—DR RUTHERFORD HALDANE.

ARTICLE V.—*Dissection of a Recent Case of Colles's Fracture of the Radius, with Remarks on the Pathology.*

By JOHN CHIENE, F.R.C.S.E., Lecturer on Surgery and Assistant-Surgeon, Royal Infirmary.

(*Read before the Medico-Chirurgical Society of Edinburgh, 4th March* 1874.)

DISSECTIONS of recent cases of fracture of the lower end of the radius are rarely obtained; the following case may therefore present some interest:—

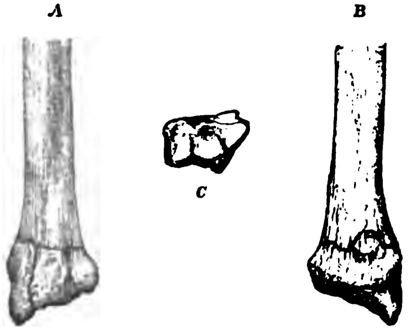
E. T., female, aged 60, in consequence of a fall from a window two stories high, sustained the following injuries:—A compound comminuted fracture of the false pelvis, with injury to the abdominal viscera; a compound fracture of the right humerus, opening into the elbow-joint; and a simple fracture of the left radius, with all the symptoms of "Colles."

The patient lived for three days. After death I had an opportunity of examining the left forearm. I am unable to say if any attempt at reduction was made during life. When I saw the case, short anterior and posterior splints were applied and kept in position by a tight bandage. The case was typical as regards the symptoms. A dorsal swelling on the back of the wrist, a hollow above it, an anterior projection on the lower part of the forearm opposite the hollow posteriorly, the carpus and hand drawn to the radial side, the styloid process of the ulna projecting at the ulnar edge of the wrist, and the disappearance of the normal projection of the head of the ulna on the back of the wrist. There was no difficulty in restoring the normal shape of the arm; the characteristic distortion could be replaced with equal ease.

Soft Parts.—Extensive extravasations of blood, both superficial and deep, extending on the posterior aspect of the arm to within two inches of the elbow-joint. Extravasation, specially marked on the sheaths of the extensor tendons of the thumb and carpus, and at the origins of the flexor longus pollicis, and deep flexor of the fingers. No tendon was ruptured, and the principal strain was apparently on the flexors of the thumb and fingers. The lower fourth of the pronator quadratus was torn across, and no fibres of this muscle remained adherent to the lower fragment of the radius.

Bony and Ligamentous Structures.—An examination of the accompanying woodcut shows the extensive splintering of the lower fragment into the wrist-joint. (A careful examination of radii which have united with deformity after this fracture, indicates that the bone is frequently splintered into the wrist-joint. Stiffness of the wrist, a most persistent and annoying sequela, is, in such cases, to be attributed to this complication, and not, as is generally supposed, to inflammatory adhesions taking place in the flexor and extensor sheaths. These adhesions account for the stiffness of the fingers, so often observed when the splints, extending to the tips of the fingers, are kept on for too long a period after the occurrence of the fracture.) The periosteum remained entire, except on the posterior aspect of the bone, and there was therefore little separation of the different splinters. There was a slight upward obliquity in the line of fracture, from before backwards, and from the ulnar to the radial edge of the bone. As far as diagnosis during life is concerned, we may consider the fracture transverse both in the antero-posterior and transverse diameters of the forearm. The line of fracture anteriorly was 4-10ths of an inch, posteriorly 7-10ths of an inch, from the extremity of the bone. These measurements make the fracture almost transverse, because the anterior length of the normal radius is less than the posterior length. The posterior surface of the fragment had no osseous connexion with the mass of the lower fragment. The scale of bone so separated was divided into three portions by two vertical cracks. On looking at the carpal surface, in addition to the fissure running along its posterior border, there was a portion of the centre of the surface separated from the rest, and driven at the same time upwards into the cancellated tissue. The tip of the styloid process of the ulna was broken off. The triangular ligament joining the radius to the base of the styloid process of the ulna remained entire, and by it the lower fragment of the radius was firmly united to the ulna.

Fig. 1.

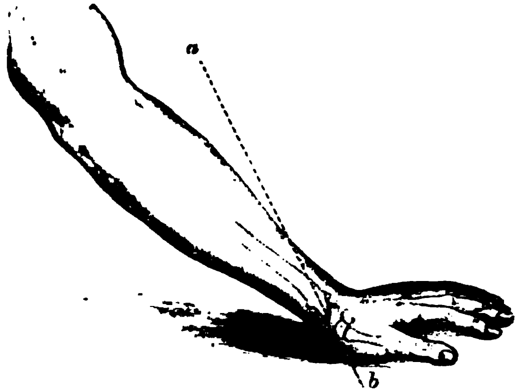


Lower Extremity of Broken Radius.
A, Posterior surface. B, Anterior surface. C, Carpal surface.

The lower fragment exhibited the triple displacement commonly described. 1. Backwards, as regards the antero-posterior diameter of the forearm. 2. Rotation backwards of the carpal surface on the transverse diameter of the forearm. 3. Rotation through the arc of a circle, the centre of which is situated at the ulnar attachment of the triangular ligament, the radius of the circle being a line from the ulnar attachment of the triangular ligament to the tip of the styloid process of the radius. It is here necessary to consider the cause of this complicated triple distortion, which occurs when

a person in falling puts out his hand to save himself, fracturing hi radius.

FIG. 2.



Position of arm when person falls forwards, throwing out his hand to save himself. *a b*, line representing direction of force, passing through the ball of thumb, carpus, and lower end of radius.

1. When the hand reaches the ground, the line representing the direction of the force, received principally by the ball of the thumb, passes into the carpus, and thence into the lower end of the radius. If at the moment of impact the angle between the axis of the forearm and ground is less than 60°, the line representing the direction of the force passes upwards *anterior* to the axis of the forearm; the whole shock is therefore borne by the lower end of the radius, which is broken off, and the force being continued, the lower fragment is driven backwards. From a series of experiments, I am of opinion that the angle, at the moment of impact between the arm and the ground, is, in the majority of cases in which a person falls forward and puts out his hand to save himself, about 45°. Hence the frequency with which the radius is fractured immediately above its carpal extremity. When at the moment of impact the angle is greater than 60°, then the line of force, instead of passing *anterior* to the axis of the arm, passes up the arm, and the usual result is either a severe sprain of the wrist, or dislocation of the bones of the forearm backwards at the elbow-joint.

2. The carpal surface of the radius slopes forwards, and therefore the posterior edge of the bone receives the greater part of the shock; there is as a result rotation of the lower fragment backwards on the transverse diameter of the forearm.

3. The carpal surface of the radius slopes downwards and outwards to the radial edge of the arm; therefore the radial edge of the bone receives the principal part of the shock through the ball of the thumb. As a result this edge of the lower fragment is displaced upwards to a greater extent than the ulnar edge of the fragment,

which remains firmly attached to the ulna by the triangular ligament. The fragment of bone, in other words, rotates through the arc of a circle, the centre of which is the ulnar attachment of the triangular ligament, the radius of the circle extending from this point to the apex of the styloid process of the radius.

Relations of the Lower to the Upper Fragment.—The upper fragment had passed into the lower fragment, crushing the cancellated tissue. *There was well-marked penetration, but no impaction.* I make a distinction between penetration and impaction. Penetration is the passage of the one fragment into the other; when the distortion cannot be remedied by any justifiable force by the surgeon, then impaction has occurred. In every case of fractured radius with displacement backwards of the lower fragment, there is more or less penetration of the upper fragment into the lower, more especially towards the radial edge of the arm.[1] This dissection, other described cases of similar dissections, and the examination of radii in which this fracture has taken place, and which have united with deformity, seem to me to prove this. The normal radius is longer posteriorly than anteriorly. In a radius which has united with deformity after this fracture, the posterior length is in the great majority of cases less; in some few cases equal to, but is never of greater length than, the anterior measurement, as is the case in the normal radius. It is impossible to believe that after this injury the anterior surface of the bone is lengthened; we must therefore come to the conclusion that there is shortening of the posterior surface, and necessarily overlapping of the fragments. It can, however, never be more than half an inch, and that only to the radial edge of the arm if the ulna is unbroken, and the ligament joining the radius to the ulna remains entire; if the ulna is broken, or if this ligament is ruptured, then the triple displacement already described cannot occur, the case is not one of Colles's fracture. An interesting question here arises: Can this penetration account for the impaction, sometimes so great as to prevent the surgeon restoring the radius to its normal shape? When we remember the great traction that can be applied to the lower fragment by pulling on the hand, it is in my opinion highly improbable that the penetration, never more than half an inch, and that only at the radial edge of the arm of the upper fragment, into the friable cancellated tissue of the lower fragment, can account for these cases of firm impaction. I am aware that this is the generally received opinion, and therefore beg, with diffidence, to suggest an explanation which seems to me to account more satisfactorily for these cases of firm impaction. I have already tried to show that this fracture occurs in consequence of falls on the hand, when the angle formed by the axis of the arm and the ground is less than 60°, generally about 45°. When at the moment of impact the angle is greater than 60°, then the result (as already mentioned) is usually a sprain of the wrist or dislocation at the elbow. If, however, in such cases the radius gives way, it will be by the upper rounded articular surface of the carpus acting as a wedge and splitting the lower end of the radius. A continuance of the force drives the carpus upwards into the cancellated tissue of the radius; it there becomes locked between the anterior and posterior surfaces of the bone, and will in such a position be firmly impacted. It is grasped by the radius, and held in position by the flexor and extensor tendons surrounding the bone. The more the surgeon pulls, the more tense do these tendons become, and the more firmly the carpus is held in its abnormal position.

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[1] The fracture occurs at the moment that the hand reaches the ground, in the case of a person putting out his hand to save himself when falling forwards. The hand is fixed; the body passes forwards, the arm becomes more vertical, and as a necessary result of the weight of the body, acting through the arm, penetration of the upper into the lower fragment must take place.

Smith of Dublin has denied that there is penetration, while Voillemier, and more recently Callender, hold that there is always penetration. While agreeing with them that penetration always occurs, I am of opinion that this penetration, as held by them, cannot account for these cases of firm impaction. The question can only be settled by the dissection of a recent case of impacted fracture, in which the surgeon has previously failed, during the life of the patient, to reduce the deformity. I am not aware that any such case has as yet been described.

The primary penetration is the result of the direction of the force causing the fracture; a return of the deformity, in consequence of improper treatment, is due to muscular action. Too much stress has been laid on the action of the supinator longus and the extensors of the thumb; they no doubt act, but it must be remembered *that all the muscles (flexors and extensors) which pass from the forearm to the carpus and hand to the radial side of the ulnar attachment of the triangular ligament, assist in causing the deformity.*

There is no change in the position of the ulna. The normal projection of the head of the bone on the back of the wrist disappears, in consequence of the passage backwards of the carpus and hand. The projection of the styloid process of the ulna at the ulnar edge is due to the passage of the carpus and hand to the radial side, as already described.

I have never been able to satisfy myself that the transverse diameter of the forearm above the line of fracture is diminished. If this is the case, then there is no appreciable adduction of the radius to the ulna by the pronator quadratus. The projection on the anterior surface of the forearm is, in my opinion, only apparent,[1]

[1] The sternal fragment of the clavicle, in fracture, is apparently tilted upwards; but, as is well known, there is no real change in the position of this fragment, the passage of the acromial fragment downwards, in consequence of the weight of the arm, fully accounting for the apparent tilting upwards of the sternal fragment. The anterior projection in Colles's fracture is explained in the same way.

in consequence of the passage backwards of the lower fragment, and not, as is generally supposed, by the pronation of the long upper fragment by the pronator quadratus, which will be counteracted by the powerful supinator action of the biceps. The locking of the fragments, which I have tried to show is a constant occurrence, will also prevent any appreciable pronation of the upper fragment by the pronator quadratus. Lastly, the flexion of the fingers, always observed in this fracture, is easily accounted for by the fact that the flexors are more powerful than the extensors. Flexion is the position of rest.

Treatment.—It is not my intention on the present occasion to enter into the treatment of this common fracture. It is worthy of note, however, that each surgeon has a special plan of his own; and, as far as I can judge, the apparatus employed by him is constantly being given up for, in his opinion, a more reliable means of treatment. The explanation of this is, either that we do not yet clearly understand the true pathology of this fracture, which can be the only true basis for a thorough and efficient plan of treatment, or if we understand the pathology, we have not yet been able to apply our knowledge so as to prevent deformity during the cure. The former hypothesis seems the probable one, and in this paper I have endeavoured to clear away some of the pathological difficulties.

In conclusion, I may state that the common plan of treatment, by straight anterior and posterior splints, has not in my hands been followed by good results. The objection seems to me to be the anterior splint, which cannot, however well padded, be applied so as to accommodate itself to the convex anterior surface of the radius. If the bandages are applied sufficiently tight to keep the splints in position, the anterior splint must, by its pressure on the anterior surface of the lower fragment, on the carpus, and ball of the thumb, tend to counteract the action of the posterior splint, which is the *essential* in the treatment of these cases, thickly padded over the lower fragment, so as to compel it to pass forwards, the bandage being applied from above downwards, so as to remedy the rotation backwards of the lower fragment. The posterior splint should only extend to the bases of the fingers; the anterior splint should only pass downwards to the lower end of the upper fragment, or if it passes to the palm of the hand, the portion of it corresponding to the normal projection of the lower end of the radius, carpus, and ball of the thumb should be removed, so as to have no counter pressure on these projecting points acting in opposition to the posterior splint. The fingers should be free from the commencement of the treatment; they should be moved by the surgeon daily, in order to prevent stiffening, which is easily prevented by these simple means, but most difficult to cure after it has once occurred.

Part Second.

REVIEWS.

Lectures on Fever. By WILLIAM STOKES, M.D., D.C.L. Oxon., F.R.S., etc. London: Longmans, Green, and Co.: 1874. Pp. 459.

ON whatever subject Dr Stokes may choose to prelect, his utterance must always be listened to with attention and respect. Fifty years of intelligent experience have well entitled him to this. But though we are bound respectfully to listen to his statements, our duty to our professional brethren and to our fellow-men forbids that we should accept his conclusions, except in so far as they are consistent with truth; and Dr Stokes is too honest a man, and too truly a truth-seeking physician himself, to expect us to do otherwise.

Dr Stokes is not long in throwing down his gauntlet; for when, at page 19, after referring to the revolution in the treatment of disease, he says, "This change in practice, depending on change in the vital character of disease," he must be well aware that he makes a statement not likely to pass unchallenged, involving as it does to a very great extent the whole question of whether we have during the last thirty years made any progress in our knowledge of pathology and of disease generally. We are not of those who regard the doctrine of change of type in disease as a mere invention to cloak former errors on the part of our predecessors, because we know that it has been a prevalent delusion from the earliest ages; and just as phlebotomists and anti-phlebotomists have flourished side by side from the days of Galen and Erasistratus, and even before them, so we have had, in the very earliest days of medical science, Asclepiades and others putting forward change of type as a reason for not bleeding, and Soranus and others arguing that change of type was a mere delusion. In support of his views, Dr Stokes quotes, with approval, "*a Memoir*," by Sir Robert Christison, "*On the changes which have taken place in the constitution of Fever and Acute Inflammations in Edinburgh during the last forty years*," published in the numbers of this Journal for January and July 1858. Amongst the fevers there described, we find what Sir Robert has termed *synocha* as a fever characterized by "a truly sthenic, inflammatory, or excited state of the pulse," which was the prevailing type in 1817-20, true typhus having been then very rare. According to Sir Robert this form of fever was characterized by a temperature "ranging from 102 to 105, at times even to 107," while the pulse varied from 120 to 160, and its replacement in modern days by typhus was apparently regarded by him as a crucial instance of change of type in fever. But change of form is not change of type; to prove that, it would require to be shown that synocha, when it did return, had itself put on an asthenic type. In our number for July 1870, however, we have an account, by Dr Claud Muirhead, of synocha as it appeared in Edinburgh during that year, and we find that the pulse in adults, between thirty and forty years of age, ranged from 120 to 130, while the temperature varied from 102 to 104, and was occasionally over 106,—facts which conclusively prove, that so far as symptoms and physical signs go, there was no difference between the type of synocha in 1817 and that in 1870. But our pathological views had considerably changed; we no longer regarded these phenomena as proofs of the sthenic character of the disease, nor did we treat it after the heroic fashion of our forefathers; and we may add with all humility, that the success of our treatment was not less than theirs. And though this so-called sthenic character of the last epidemic of relapsing fever is thus quite sufficiently proved by the figures ascertained in the Edinburgh Infirmary, it is more than proved when we cast our eyes a little further afield; for, by careful observations made—often every two hours—in the London Fever Hospital, as well as by many accurate observers in Germany, the temperature during the last epidemic of relapsing fever occasionally was found to go up nearly to 109° F. (actually to 108·7°). The rapid, full-bounding pulse is characteristic of all epidemics of relapsing fever, not only of those of 1817–20; and had we bled our patients, the venous blood would, we doubt not, have been as florid, and flowed with just as much force (almost *per saltum*) as formerly. But our patients may congratulate themselves that the present generation of physicians do not associate the idea "sthenic" with a bounding pulse and a high temperature; and without casting any slur on our predecessors, who acted honourably up to their lights, we may be permitted to quote their own figures, and to say that, of 364 cases bled from the arm (we quote from Dr Welsh's account of the epidemic of 1819), 20 died, or 1 in 18·2; while of 190 cases neither bled nor leeched, 4 only died, or 1 in 47·5; while during our last epidemic, which, though not very extensive, was yet large enough to afford, under appropriate treatment, a moderate mortality, not even leeching was employed, and only one man died (Muirhead). In 1853, Sir Robert Christison said: "My own convictions on the subject are so strong, that I regard nothing as more likely than that, in the course of time, some one now present will see the day when a reflux in the constitution of fever will present it again in its sthenic dress, and again make the lancet its remedy." Little more than ten years after this, the first part of this prophecy was fulfilled; fortunately for our patients, the last was not.

Sir Robert Christison's conclusions are to the effect that the attention of practitioners was first drawn to a change in the constitution of epidemic fever; that they were thus led to observe an analogous change in the constitution of the symptomatic fever attend acute inflammations, and to alter their treatment accordingly. B an apparent change of type did not progress *pari passu* in all f of epidemic fevers, for Sir Robert himself mentions an asth type of measles alongside of a highly sthenic type of fever in eve other class of febrile disease (July 1858, p. 41); and Dr Ali states, in a clinical lecture on 7th May 1850, "that the inflamma-tory epidemic of continued fever observed from 1815 to 1818, wa coexistent with a more typhoid form of measles than we have ev seen since;"[1] while Sir Robert Christison, speaking of the sam epidemic, says, that "Dr Gregory, in his lectures in mid-winter of 1817–18, mentioned that, for the first time since 18 he had seen a few similar cases, in which antiphlogistic treatmen could not be sustained. My own earliest acquaintance with measles, during the summer of the same year, was quite different. Both then, and for many years afterwards, it put on the same sthenic form . . . . which characterized our continued fevers of the same period." Now, whether this be a marked instance of the influence of mere medical opinion in regard to the existence of a certain type in disease or no, it matters not, for it is evident enough from the statements quoted, that a change of type in measles is not by any means always coincident with any similar change of type in continued fever. And the same thing may be said in regard to sporadic diseases, for some yet alive must distinctly remember the epidemic of diphtheria which occurred about fifty years ago, when the practitioners of those days poured wine into their diphtheritic patients with one hand, while with the other they bled largely and freely those affected with pneumonia. But this, though true, is indeed a matter of but small moment, as Dr Alison himself has stated that all arguments drawn from the supposed existence of varying types in epidemic fever are utterly irrelevant when applied to sporadic or stationary diseases.[2]

Many competent observers, and amongst them Murchison, are satisfied that any apparent change of type in epidemic fever has been actually a change in the disease. This opinion is not homologated by Dr Stokes; and it is in a great measure to his peculiar pathological views that we owe his determined adhesion to the obsolete theory of change of type. Any one may well be excused for holding such views who has actually the assurance to propound the doctrine in this year of grace 1874, and after all that has been written on the subject, that "In 'relapse cases'—I do not mean cases of relapsing fever—the character of the second attack may differ widely from that of the first. Thus typhus may give place to typhoid, or *vice versa*" (Preface, p. 71). But this idea may be carried still further, for relapses following typhus may put on, not the form of typhoid only, but even of scarlet fever, and, in favourable circumstances, of smallpox. In former days, when all fevers were jumbled together promiscuously in the wards of our Royal Infirmary, we have known patients who relapsed, as Stokes calls it, twice over before they got out, and each time the relapse had a separate type; the original disease might have been typhus, typhoid, or scarlet fever; the relapses were the other two in irregular succession. But these peculiar relapses led to no vague speculation as to the identity in essence of all forms of fever, but to the more practical result of the more careful segregation of the various forms of fever, so that nowadays such relapses are practically unknown. Nay, more, for years past we have been constantly in the habit of introducing typhoid patients into the general medical wards, with apparently beneficial results to the fever patients themselves; and by attending to the ordinary precaution of disinfecting the excreta, we have never had one single case originating in the wards, either in the original or any other type. On the other hand, typhus and scarlet fevers are carefully shunned, and if either one or other accidentally gets into a ward for one night only, as has occurred though rarely, it has happened that the patients suffered, the original type having been reproduced in one or other of those who happened to be next the infected bed. In contrast to this, and as affording proof of the peculiar way in which these fevers propagate themselves, we may mention that in former days, when every patient even suspected of fever was at once ushered into one or other of the fever wards, with which of course the patients in the general wards had no communication, except that both used the same water-closet, the same care in disinfecting the excreta not being then employed, typhoid fever not unfrequently broke out in one or other of the patients in the general ward—typhus almost never; and if it did, it could always be readily traceable to actual contact.

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[1] Edinburgh Medical Journal for August 1850, p. 165.

[2] Ibid., May 1857, p. 975.

In those days, it was often remarked by the attending physicians, that those patients who had typhus following typhoid invariably, or almost invariably, had their crisis on the tenth day, and therefore had a milder form of fever than usual; this, of course, leading up to the question of the possible identity in essence of all forms of fever,—a doctrine which was held in its widest sense by Alison, and which, to a greater or less extent, is held by Stokes, Kennedy, and many other of the Irish physicians. This doctrine is not only plausible, but possible. Far from disavowing it, we accept it as extremely probable; but that does not in any way alter our views as to the practical differences between the various forms of fever as they now exist, and as to the necessity for a diversity in the conduct and treatment of all of them. We may accept Samuel Brown's views as to the possible transmutation of metals, and the possible identity of many, if not all, of our so-called elements, as not only plausible but even probable; but that belief has not the slightest influence in our practical ideas as to the relative value of the metals.

The two theories have much in common—they are both hig philosophic; they are both thoroughly unpractical.

Remembering, then, that Stokes is thoroughly biassed in fav of change of type, and of the identity of typhus and typhoid fevers—views which we cannot homologate, and which we conceive to altogether at variance with the teachings of modern pathology—there is yet much to commend in this volume. It is well print agreeably written, and pleasant to read, and, above all, it conta the life-long experienee in fever of a most accomplished physicia "all of the olden time," and much important and valuable info-mation may be gleaned from its pages. It is a work which w repay the careful study of an educated and intelligent physician, but scarcely one which we would put into the hands of a student for the purpose of forming his views as to fever.

Fifteenth Annual Report of the General Board of Commissioners in Lunacy for Scotland. Edinburgh: 1873.

In this blue-book we have 312 pages devoted to details abou the management of the insane in Scotland. The Commissioners have been at their work all over the country, noting the deficiencies of asylums and the advantages of the boarding-out system, and keeping steadily in view the interest of the unfortunate class over whose welfare they have so much influence. The book is principally made up of reports upon the present condition of district and private asylums, lunatic wards of poorhouses, and other establishments for the charge of the insane. It certainly cannot be said that the statistics are too scanty, though they are often incomplete for a logical conclusion to be deduced from them. On their interpretation we are pleased to see that the Commissioners look with due caution, and they have not neglected to compare them with documents furnished by the Board of Supervision and the Registrar-General and other cognate sources. The number of insane persons in Scotland known to the Lunacy Board maintained from private sources is 1393; the number of pauper lunatics is 6405; and 51 are kept at the expense of the State. The number of unreported insane in private houses is computed to be about 2000.

The following passage is of some general interest:—"The number of pauper lunatics will always largely exceed the number of the private insane, for pauper lunacy is, in a very large proportion of cases, the expression of incapacity for independent productive labour. The tendency of those whose brains are abnormally constituted, if without hereditary fortune or friends willing to assist them, is to sink step by step through the various grades of society, until they reach the final sedimentary deposit of pauper lunacy. The conditions which promote this descent are simply those which destroy health; namely, abuse of the passions, intemperance, unhealthy habitations and occupations, improper diet, overwork, and deficient exercise and recreation. The increase of insanity is not, we maintained, a result of modern civilization, save in so far as modern habits have led to a departure from the conditions of healthy existence, by overcrowding in cities, by exhausting labour, by breathing vitiated air, by over-indulgence in stimulants, by inappropriate food, and by neglect in the training of children."

It seems likely that those who hold that an increase of insanity is the result of modern civilization, will be disposed to admit that exhausting labour, overcrowding in cities, over-indulgence in stimulants, and the other causes mentioned in the passage quoted, are really the causes which make man in a highly civilized community more likely to become insane than in a less civilized one. These evils go along with our present form of civilization, and the problem is how to separate them from it.

The Commissioners point out that "69·54 per cent. of our population live in houses of one or two rooms, while 82·11 per cent." (we suppose 82 per cent. taken from the remaining population) "live in houses of three rooms and under." The attempts to build the working classes larger houses have not succeeded. Instead of occupying the additional rooms in the new houses provided for them, the people let them to lodgers, "and, in order to spare money for dress and better food and drink, they never hesitate to crowd their families into as confined a space as possible that they may sublet one or two rooms."

"The more these details are considered," adds the Report, "the more hopeless will appear the task of remedying the evils which they shadow forth, until measures be adopted for raising the character of the masses, and training them to higher aspirations. And success in this object would involve, not merely an extension of school teaching to every member of the community, but a radical change in our ideas of the nature of education. The important fact must be recognised and acted upon, that the moral faculties of the human mind require to be as carefully trained as those of the intellect; and that without moral training, intellectual training may but serve to increase the aptitude to do wrong."

This is somewhat vague, but we have an explanation further on. "Every man, when properly qualified, must be regarded as his own best guardian; and until every man is fitted by appropriate education and training to discharge this duty towards himself, we may lay our account with an increase and an intensification of those manifold evils which degrade our intellectual, moral, and physical nature, and which, among other lamentable results, bring insanity in their train. The compulsory instruction of the people, which is, we trust, likely soon to be accomplished, cannot fail to prepare the way for a reform in the mode and scope of education; but we fear it is not yet seen that mere scholastic teaching, which does not include moral training, and a knowledge of the laws which regulate the bodily and mental health, is in reality no safeguard against either crime or insanity."

The change which is advocated apparently is, that more attention should be paid to moral training at schools, and that the children should be taught the laws which regulate the bodily and mental health, and if this be done a check in the increase of insanity may be expected. We are not so hopeful of such a result. Morals are best taught in the homes of the parents by precept and example, and the general laws which regulate health have been known with an exactness sufficient for practical purposes before the days of Hippocrates. Long before physiology was a science, people have been aware that crowded cities and narrow houses are bad for the health, that drunkenness and other excesses ruin the constitution, and that if men do not eat well they will not work well, and will not live long. There are few mothers who do not know that it is better for the health to have a good bedroom for the children than to have a fine bonnet or a silk gown, and that drugging them with laudanum to keep them quiet during factory hours makes them puny and liable to disease. No one requires to read a book on popular physiology to be taught all that; nor do we see how a few hazy and inexact notions about the lungs exhaling carbonic acid, and the capillaries of the brain or of the liver becoming wider when alcohol gets into the blood, will make a man so much the less inclined to drink, or a woman to take in lodgers, that we may expect from this species of instruction a marked diminution in the prevalence of insanity. Lunacy is one of the vast evils that afflict mankind, and we fear there are no ways of preventing it at present, save by those general influences which we hope will in time do something to raise the condition and improve the moral tone of society, and among these influences no doubt education, both intellectual, moral, and physical, has its own part to play.

There are two interesting Reports by the Deputy Commissioners, Dr Paterson and Dr Sibbald, upon the condition of single patients. As most of our readers must be aware, the General Lunacy Board of Scotland, in imitation perhaps of the successful experiment of the Board of Supervision in the boarding-out of pauper children, have carried out on a considerable scale the plan of sending insane patients thought to be harmless to live with such as were willing to take them for the sum of sixpence a day. This was a pet scheme of the Board of Lunacy, and the indulgent comments which were made upon the out-door lunatics sometimes stood in strong contrast to the hypercritical strictures that fell upon the appearance and food of those in the great asylums of Edinburgh, Glasgow, and Dumfries, where veteran superintendents were deemed not to be sufficiently attentive to the suggestions of the Government Commissioners. We were never good converts to the boarding-out system, and knew well enough that the number of people willing to take mad men or mad women into their houses was likely soon to find a limit; but there was no doubt that many patients preferred the liberty they enjoyed in the cottages where they were boarded, to the dreary corridors and airing courts and guarded fields of the district asylums, even although their food might be poor and scanty, and the bedding such as would bring down an unfavourable entry if seen in an asylum, and though they might be now and again badly treated, put to servile work, or neglected when ill. When they got troublesome they were of course sent back to the asylums. The reports of the Deputy Commissioners were always highly favourable to the boarding-out system, and these reports excited interest not only in the southern kingdom, but also on the Continent and in America. It would certainly be a pity if so interesting an experiment were cut short; and it seems to us, if the proposal of the Chancellor of the Exchequer to give four shillings a week from the consolidated fund for each pauper lunatic sent into a district asylum be carried, that the boarding-out system will have received its deathblow. The boarding system is a considerable saving to the parish rates, but here we have a bounty of four shillings a week put upon paupers in district asylums.

Another evil well known to the medical superintendents of these establishments has been pointed out by one of the Commissioners in an entry in the visiting book of the Montrose Royal Asylum. "Among the cases admitted are many which require only careful nursing, and which might equally well be provided for in the wards of a poorhouse or in an infirmary; but the facilities for obtaining certificates of lunacy, and for disposing of patients in the Asylum, lead to what may in a certain sense be regarded as an abuse of the institution. Should the proposal embodied in Mr Crawfurd's Bill for the Amendment of the Poor Law, to make all pauper lunatics a charge upon the consolidated fund, receive the sanction of the Legislature, this abuse will be infallibly increased to an enormous extent; while the counteracting influence of removal by minutes of parochial boards will become inoperative by the withdrawal of the pecuniary motive to interfere."

Should the four shillings a week not be extended to parochial asylums and the lunatic wards of poorhouses, a great deal of money will have been thrown away by those towns who have erected such buildings, which in their extensive scale and management are well fitted for the reception of the insane, though under parochial management. It may be noticed that the district asylums in Scotland are not under the direct management of the ratepayers, but, like the roads and jails, are looked after by committees of country gentlemen formed into what are called District Lunacy Boards. These managers, being resident in the counties and influenced to a certain extent by the opinions of their neighbours, are the only parties who have an interest in keeping down the expenditure in the district asylums; but this interest will in great part disappear if the unhappy proposal of the Chancellor of the Exchequer be not given up. A su of money given from the Imperial Exchequer to lighten the heav burden of taking care of the insane in the Orkney and Shetla Islands, and the poorer districts of the North, would be a use grant; otherwise we see no reason for a change in the collecti of the rates, which in any case must come out of the pockets of th ratepayers, whether collected through the Inland Revenue o the Parochial Board.

In conclusion, we may say with some pride that the Scottis Lunacy Report, both in matter and in literary treatment, wi bear a favourable comparison with those of England and Ireland. Perhaps it would be well to imitate the Irish Report, and make it a little shorter.

Thirteenth Report of the Scottish National Institution for the Edu-cation of Imbecile Children at Larbert.

THIS institution is steadily advancing year by year in the number of poor children it educates, and therefore in its usefulness to humanity. There are now ninety patients there. Any commendation of the work done there, with such a man as Dr Ireland at the head of it, would be simply supererogatory. Our scientific knowledge of idiocy is widening materially, as the result of his careful observations and deductions on the subject, and this is the best proof that he devotes himself to his work with zeal and success.

The Asylum is in need of more funds, and we are satisfied, from a careful perusal of this Report, that its small income is most judiciously spent. Surely a liberal public will not let such an institution starve. It appeals especially to the sympathy and benevolence of woman. The cry of helpless children was surely never heard by her in vain, and in the case of the weak-minded inmates of the Larbert Institution they present the truly pitiable spectacle of being helpless after infancy has passed, and of remaining helpless as children all their lives, if not trained and schooled on principles applicable to themselves alone. Our profession, among its many works of benevolence, will find few more deserving or needed charities than this to which to direct the attention of their kind-hearted and liberal lady patients. Let them read this Report for themselves.

Annali di Chimica applicata alla Medicina. Compilati dal Dottor GIOVANI POLLI, Milano.

WE have to acknowledge several numbers for 1873. This periodical gives a good report of the principal observations made in pharmacy, chemical physiology, and therapeutics.

We have a number of contributions from Italian physicians, as well as translations or résumés of the most important papers published all over Europe and America. Among the contributions coming from Italy itself, there is one in the number for November upon the necessity of instituting researches upon the venom of hydrophobia, by Professor Tigri, in which he asserts that there are microzoa found in the venom of the viper, as also in that of the scorpion. There is also an interesting article on palustral miasma, and the way to preserve oneself from it, by Professor Antonio Selmi, in the number for February.

There is also in the numbers for October and November an elaborate paper by the Editor, upon the cure of calculi through means of chemical solvents, which may in many cases be preferred to surgical operations.

We have several articles upon the properties of the Eucalyptus globulus to prevent malarious fever. There is a long one on this subject translated from the French of Figuier, and another in the May number, by Professor Binz, on the febrifuge action of preparations of this remarkable plant.

In the July number there is a note upon the treatment of ague by means of hypodermic injections with carbolic acid as used by Declat. Five grammes of water, with carbolic acid in the proportion of one to a hundred, are used for four injections during the first day. Three injections are used the second day, and two the third. The injections are practised on the breast or sides. The author maintains that this treatment is superior to quinine. There is also a valuable paper on ozone and antozone, by Gugler and Nesse, in the June number. M. Daeverne recommends lime-water to relieve the pain caused by the stings of wasps and other insects. The following is strongly recommended as an anti-herpetic liniment, and may be tried in a disease which is sometimes rebellious to treatment:—

Hydrate of chloral, . .	2 parts.	Fresh butter, . . .	10 parts.
Borate of soda, . . .	3 „	Almond oil, . . .	5 „
Sulphate of soda, . .	3 „	Essential oil of lavender, .	2 „
Rose-water, . . .	10 „	Glycerine, . . .	5 „

The ingredients to be added in the same order as here arranged.

Cremation: The Treatment of the Body after Death. By Sir Henry Thompson. Pp. 55. London: King: 1874.

Sanitary Arrangements for Dwellings. By William Eassie, C.E. London: Smith, Elder, and Co.: 1874.

Sir Henry Thompson, in republishing his now celebrated review article, takes an opportunity of answering the objections urged against cremation by Mr Holland, the Government Inspector of

Churchyards, and does so very successfully. The special objecti to the process, and the only one that appeared to us of any we —viz., the risk of criminal poisoning remaining undetected ow to the dissipation of the poison—is met by Sir Henry insist 1*st*, On a thorough verification not only of the death, but a of its cause; and, 2*dly*, In case of any suspicion, on the preservation of the stomach, etc., for the purposes of analysis. "To su up:—For the purposes of cremation nothing is required but apparatus of a suitable kind, the construction of which is we understood and easy to accomplish; with such apparatus the process is rapid and inoffensive, and the result is perfect. The spa necessary for the purpose is small, and but little skilled labour is wanted. Not only is its employment compatible with religious rites, but it enables them to be conducted with greater ease, an with far greater safety to the attendants than at a cemetery. For example, burial takes place in the open air, and necessitates exposure to all weathers, while cremation is neccssarily conducted within a building which may be constructed to meet the requirements of mourners and attendants in relation to comfort and taste. Cremation destroys instantly all infectious quality in the body submitted to the process, and effectually prevents the possibility of other injury to the living from the remains at any future time. All care to prevent such evil is obviously unnecessary, and ceases from the moment the process commences. The aim of cremation is to prevent the process of putrefaction. On the other hand, burial cannot be conducted without various risks to the living, and great care is required to render them inconsiderable with our present population. Costly cemeteries also are necessary, with ample space for all possible demands upon it, and complete isolation from the vicinity of the living, to ensure, as far as possible, the absence of danger to them. It is a process designed essentially to prolong decay and putrefaction, with all its attendant mischief; and the best that can be affirmed of it is, that in the course of many years it arrives, by a process which is antagonistic to the health of survivors, at results similar to, but less complete than, cremation produces in an hour without injury to any." Such are the conclusions of a remarkable pamphlet, which, both in this country and on the Continent, has deservedly attracted much attention to a subject of great sanitary importance. The question is rapidly becoming a popular one, and there can be no doubt the more it is discussed the disadvantages and dangers connected with burial will be the more apparent, and the benefits of cremation will be recognised.

Mr Eassie, the author of the second contribution to hygiene on our list, is, strange to say, the secretary of a society formed in London to carry out Sir H. Thompson's views as to the disposal of the dead. He is a well-known sanitary engineer, who some years ago published a very sensible and useful little work, entitled "Healthy Homes," of which indeed the present volume appears to be a condensed and improved edition. The *British Medical Journal* has the credit of introducing a large portion of it to its readers in the shape of a series of short papers. In its present form, it is, in our opinion, a very sensible work, sufficiently explanatory to be understood by the non-professional reader, and calculated to be of use to all medical men. Mr Eassie freely acknowledges the shortcomings of builders and architects, and confesses that the members of the medical profession "have ever been foremost in pointing out the danger of many practices which were unwittingly prolonged by their architectural brethren. The uselessness of water-sealed traps when the air of the sewers is in a compressed state; the necessity of ventilating house-drains; the danger of trusting to well waters, except under certain circumstances; the reduction of phthisis consequent upon a well-ordered system of water drainage—these are a few instances of their watchfulness." We think our author might have added to this catalogue the necessity of disconnecting the cistern overflow-pipe from the house-drains, a point of the greatest importance in the sanitary arrangement of dwellings.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING VII.

Wednesday, 6th May.—Dr P. H. WATSON, *Vice-President, in the Chair.*

Exhibition of PATHOLOGICAL SPECIMENS and MEDICAL NEWS.

I. *Dr P. H. Watson* showed—(1.) EYE OF A LAMB, in which a tuft of wool grows from the cornea, proving the cutaneous nature of this part of the eye. Dr Watson referred to Sir W. Wylde's monogram upon this subject.

(2.) EPITHELIOMA FROM THE LOWER LIP, which had been aggravated by caustic applications made by an irregular practitioner.

(3.) EPITHELIOMA FROM THE ANGLE OF THE MOUTH AND CHEEK, extending as far as the margin of the masseter muscle, requiring extensive removal of the cheek, and the performance of a plastic operation to occupy the gap thus produced.

(4.) SCAPHOID, CUBOID, AND THREE CUNEIFORM BONES, *en masse*, removed by Dr Watson's operation of excision of the anterior tarsus and base of the metatarsus.

(5.) An ASTRAGALUS removed in a case of compound dislocation of the bone. The accident which occasioned this lesion was incurred while the patient, a medical man, was stepping on the ground from his carriage; the horses moving forwards, his weight was thrown suddenly upon the anterior and outer part of the foot. The bone had evidently been wrenched away anteriorly from the scaphoid bone, and inferiorly from the calcaneum, and its anterior surface forced out through an opening in the cutaneous textures between the external malleolus and the external margin of the extensors of the toes, whereat the astragalus projected. On manipulation, the bone was found to be only attached to the extremity of the tibia and fibula by the capsular ligament of the ankle. After applying a tourniquet, the bone was excised. The progress of the patient had since this time been uninterruptedly satisfactory.

II. *Dr M'Kendrick* exhibited ten photographs of MICROSCOPIC OBJECTS recently sent him by Professor Beneke of Königsberg. These were remarkable for fine artistic finish, and were admirable examples of micro-photography applied to physiological work.

III. *Dr M'Kendrick* stated that he had recently observed a well-marked instance of PHYSIOLOGICAL ANTAGONISM BETWEEN BROMAL HYDRATE AND ATROPINE. Bromal hydrate, when administered in a dose of three grains to a medium-sized rabbit, produced contraction of the pupil, dilatation of the bloodvessels, profuse salivation, œdema of the lungs, and effusion of fluid into the pleural, pericardial, and peritoneal cavities, and other effects. He had observed that half a grain of atropine arrested the hyper-secretion, and in various cases saved life from what was afterwards found to be a lethal dose. The detailed results he expected soon to communicate to the Society. In the meantime, he announced the fact as another example of the existence of a true physiological antagonism between certain active substances, as had been so well shown by Fraser and others.

IV. *Dr John Duncan* then gave a brief account of a most interesting case in which he had operated for the RELIEF OF INTUSSUSCEPTION, of which the following is an abstract:—

Gastrotomy in a Case of Intussusception, æt. 5 months.—Was called by Dr Balfour of Portobello to see the following case, in which the points of interest were—1. *Diagnosis.* Precedence of diarrhœa, griping, passage of blood and mucus. These are usually mentioned. From anus the end of invagination was felt, and the firm cylinder of thick bowel could be traced from surface of abdomen.

2. *Treatment.* — Insufflation and injection were unsuccessful, because of straining keeping sphincter relaxed. Opened abdomen in middle line and found small intestine invaginated whole length of large and cæcum into ascending colon. Could not pull out, but by process of taxis, aided slightly by pulling, succeeded in relieving

it. Child rallied, and remained well for a day, and then died suddenly, apparently from syncope.

The *President* expressed the interest he felt in the case, and approved of the plan adopted by Dr Duncan of aiding the return of the intussusception by kneading and pushing from below, as well as dragging from above.

V. *Dr Gillespie* then read REPORT ON THE CAUSES OF DEATH AMONG THE ASSURED IN THE SCOTTISH UNION INSURANCE COMPANY, from 1824 to 1870, by James D. Gillespie, Esq., M.D., F.R.C.S.E., Medical Officer of the Company; and A. B. Duffin, Esq., M.D., F.R.C.P., Medical Officer of the Company, London. The report was a long one, and contained much matter of great interest, derived from numerous tables illustrating seven different subjects for investigation:—"1. The ages, divided into quinquennial periods, at which the lives lapsed, with the diseases which terminated the lives. 2. The class, order, and disease under which the cases are ranged, with the average duration of life in each. 3. The same results as regards females. 4. The same results as regards extra-premium cases. 5. The occupation of the male cases, their respective numbers, and average duration in years. 6. The occupation of the male cases, the class of disease of which they died, with the comparative rate per cent. 7. The relative number of persons in the different occupations who fell victims to particular classes of disease."

We quote some of the most interesting paragraphs, thus:—

"Since the commencement of the business in 1824 up to the year 1870, a period of 46 years, 1951 cases have become claims; these entered, on an average, at the age of 42·24 years, and terminated at 53·94, giving the average duration of insurance 11·70 years." . . .

"There is no doubt that some of the deaths entered under diseases of the nervous system might, were the facts known, be more justly entered under delirium tremens; while it is still more notorious that some of the diseases of the digestive organs and kidneys are most probably due to habits of intemperance. It therefore cannot be too strongly impressed on all connected with the selection of lives for an insurance office to deal strictly with the question of personal history as regards habits, as it very rarely happens that the assured who live freely remain any length of time on the books. It will be seen that these 25 cases lived only on an average 6·32 years, and the loss from this cause can easily be estimated; but it is not so easy to calculate the indirect loss of much greater magnitude sustained by insurance offices, owing to early deaths from diseases induced by intemperate habits." . . . . .

"Hernia only produced 4 deaths, at an average duration of 6 years. It is worthy of consideration whether such a small amount of fatality, spread over a period of 46 years, can justify a special rate. Three of the cases occurred in England; the lives lapsing at an average age of 2·67 years; while the fourth died in Ireland after being insured 16 years. Of the 3 English cases, 1 was insured at 57 with an extra of 2 years, and he died in Spain, at the age of 58, of gangrene, owing to the Spanish surgeons not interfering and performing an operation to save life; 1 was insured at 58, with an extra, and he died at 60, of strangulated hernia; and 1 died of mortification of the bowels, after being insured 5 years. Most probably the hernia was not known to exist during life. The Irish case is a peculiar one: it was ventral hernia, and was only recognised the day before death, in an old gentleman of 73. Of the 4 who died of hernia, only 2 were paying the small extra required in such cases." . . . . .

"Table 3.—This table, giving the results of the claims of female insurers, being constructed in the same manner, requires no commentary; but we may state generally that 222 female risks have become claims, being 11·38 per cent. of the whole, whose average duration was 10·65 years. The average duration of the males being 11·83, it may be well to search out the reasons why these female lives lapsed at an earlier period. This can be explained by childbirth cutting off 10 at an average duration of only 3·1 years, and the cases of phthisis dying sooner. These latter, 29 in number, lived on an average only 5·62 years, while the male cases lived on an average 6·68 years." . . . . .

"Tables 5, 6, and 7.—These three tables open up a different branch of inquiry, and are intended to show what bearing the occupation of the insured has on their prospects of longevity. As very few of the females insured had any occupation, they have been omitted from the classification. It will be seen that lawyers, bankers, etc., head the list, with an average duration of 16·29 years; clergymen coming next, with a duration of 16·06 years; while master mariners are much the worst lives for insurance, as their duration was only 5·52 years. Engineers and commercial travellers are the next lowest. The results are different, however, in each district, and it will take a much more extensive collection of data before any reliable conclusions can be drawn. In England, professors, teachers, etc., lived the longest, their average duration being 13·67 years; while wine and spirit dealers and publicans lived only 5·55 years; master mariners coming close after them with an average of 5·83. In Ireland, 2 servants top the list at an average of 16 years; 3 teachers, etc., at 15·33; and next to them we have 63 merchants and shopkeepers, who lived on an average 15·24 years. 2 master mariners lived only 2 years between them after insurance. In Scotland 6 naval men bear off the palm, living on an average 23·5. They are followed by 82 lawyers, etc., with a duration of 18·64 years; and 49 clergymen, with an average of 18·49 years. So far from the spirit trade being apparently a baneful one in Scotland, the 32 innkeepers and spirit-dealers lived an average of 16·81 years, though 4 died of delirium tremens; while

36 English cases of the same trade lived the short period of 5·55 years on an average, but only 1 died of delirium tremens. The 12 Irish cases averaged 11·83 years, and none died of delirium tremens. 10 master mariners lived only on an average 6·4 years, so that in the three kingdoms they have proved about the worst lives. Of the 18 master mariners whose policies have lapsed, 9, *or exactly one half*, were drowned soon after effecting their insurance."

The *President* conveyed the thanks of the Society to Dr Gillespie, and congratulated the members on the amount of interest which Dr Gillespie had managed to infuse into a somewhat dry subject.

Dr Matthews Duncan expressed the great interest he felt in the subject of the paper, especially as regards the whole question of deaths from childbirth, and their bearing on life insurance—a subject regarding which he had been much consulted by insurance companies—as insurance offices object, and rightly object, to receiving young unmarried or recently married women as lives at ordinary risks. There is still a tendency in some quarters to make light of childbirth as a cause of death; and insurance offices do not know, and would wish to know, how much it affects their calculations. It is well known that some great authorities—such, for example, as Miss Nightingale—say that childbirth is a natural process; that no one woman should ever die in childbirth. This is an absurd, monstrous, and nonsensical statement. The question is not whether women *should* die in childbirth, but whether women *do* die in childbirth? He (Dr Duncan) had shown that a great mortality did exist; but Miss Nightingale, and Dr Farr—a far higher authority than Miss Nightingale—repeat their statement recently regarding Dr Duncan's *frightful* statistics of the mortality of childbirth. Miss Nightingale tells that, in military practice, all that is wanted is that the woman should retire for two or three days to a hut, and all goes on perfectly well after a day or two in rest and quiet. Dr Gillespie's office experience is a very different one. The mortality of childbirth is the great reason that female mortality between the ages of fifteen and thirty is higher than the male mortality at the same age. This is chiefly due to primiparous births and their specially great mortality. Besides this, childbirth mortality increases, and explains the greater relative mortality of females at all childbearing ages; while the high mortality between fifteen and thirty is an expression of the evil influence of *first births*. Another great error in computation is to count in the childbed mortality only such cases of death as occur within four weeks after delivery. What special virtue is there in the four weeks, in which time the lethal influence is supposed to end? Many cases of pyæmia, or so-called puerperal fever, drag on beyond that time, and every one of experience knows that four weeks does not end the lethal time; hence, he would repeat, that his statistics of childbed mortality are not too high, but too low, as he intended to show in a future communication.

Dr Gillespie asked Dr Duncan's opinion as to the risk in a case he had seen that morning, in which a married couple wished to insure, the husband being twenty-three years old, and the wife thirty-nine, and now pregnant for the first time. Dr Gillespie had suggested that, though in other respects a good life, the wife should delay her insurance till after the birth of the child, as he believed that the fine of 1 per cent. generally imposed by insurance offices in such cases was quite inadequate to balance the risk incurred in a first pregnancy at that age.

Dr Duncan answered, that a first pregnancy increased the woman's risk of dying in that year enormously above the ordinary insurance table risk of the same year of age. Besides, in Dr Gillespie's case there was a still further increase of risk from the oldness of the woman having her first child.

VI. *Dr Matthews Duncan* then read a paper by Dr A.C. Campbell, of Dundee, on a case of CYSTIC TUMOUR ATTACHED TO KIDNEY, simulating ovarian disease; extirpation of kidney; recovery. Mrs S., 49, millworker, widow, noticed, eighteen months ago, swelling in the lower iliac fossa, which made rapid progress. When admitted it was freely movable and larger than a man's head. Three fluctuating points, which apparently communicated, were felt. Uterus was high in the pelvis. Ovariotomy was determined on. An incision made five inches in length; peritoneum opened; cyst exposed. It was tapped by a large trocar, but no fluid came. It was found to be of consistence of porridge; so cyst was opened, and two pints of stuff cleared out. Ovaries were found perfectly healthy and in normal situation. Tumour was found to be a cystic one, involving left kidney, which was removed, after adhesions to a portion of bowel and omentum were broken up, and vessels tied with catgut, and ureter tied. The patient made a tedious but complete recovery, during which she passed about forty ounces of urine daily, healthy and without albumen. [This paper will appear *in extenso* in a subsequent number of this Journal.]

Dr Duncan said it was obviously an erroneous diagnosis; the tumour had grown in a very peculiar way, covering the adherent bowels, which were behind the tumour, instead of, as is usual in such cases, in front of the kidney. Had it been so, a true diagnosis might have been gained. The general question of the diagnosis of ovarian and kidney tumours is a very difficult one, regarding which Mr Spencer Wells had written most admirably; but any one who is familiar with the proceedings even of the very greatest ovariotomists, will not be surprised at hearing of a bad diagnosis occasionally.

VII. *Dr Chiene* read a case of ABSORPTION of an EXOSTOSIS, which will appear in an early number of this Journal.

Dr P. H. Watson remarked on the rarity of the case, the usual result of the fracture of an exostosis being necrosis, abscess, and removal of the dead portion after the abscess is opened.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING XI.

Wednesday, 8th April.—Dr J. MATTHEWS DUNCAN, *President*, in the Chair.

I. *Dr Cuthbert* showed a rare and interesting case of STRANGULATION IN UTERO, and the following are the notes of the same:—

Mrs P., æt. 30, a light-haired, stout, and well-formed woman, the mother of several healthy children, miscarried for the second time, on 29th March 1874.[1] Her last menstruation was on 17th December, consequently the fœtus was about 3½ months old. It was perfectly healthy and well developed. No reason could be assigned by the patient for the accident. On examining the fœtus, I found the funis coiled twice round the neck, and, under the coiling, it was tightly tied in a single knot. The part of the funis from the umbilicus to the neck was completely on the stretch, and its whole length from the insertion in the placenta to the umbilicus was about 17 inches, or 2½ times the length of the fœtus. The part continuous with the fœtal circulation was round, and of a natural size, but the other part, viz., from the neck of the fœtus to the placenta, was small and cord-like.

There can be no doubt that the death of the fœtus, and, consequently, the miscarriage, was caused by the tying and coiling of the funis round the neck. Probably, it was also hastened as the fœtus grew larger, by putting the smaller part of the cord on the stretch, which would tend to tighten the knot and coils; at the same time, it would also stop the circulation.

II. *Dr M'Rae* read a paper entitled, AN OBSTETRICAL HISTORY—a Vesico-vaginal Fistula, with subsequent conceptions, which will appear in a future number of this Journal.

III. *Dr J. Matthews Duncan* read a paper entitled, NOTE ON THE CHIEF DIRECTIONS AND EXTENTS OF UTERINE SHRINKING; SPECIALLY AT THE TIME OF THE COMPLETE EXPULSION OF THE CONTENTS OF THE GRAVID UTERUS. The subject was quite a new one, so far as he was aware. Shrinking and contraction, he pointed out, were not identical. Action or contraction was the force or condition that produced shrinking. This latter term expressed one result of action or contraction.

Shrinking of the muscular wall of the uterus generally caused no rugæ or sulci in the peritoneum, this membrane being sufficiently elastic to follow the shrinking uterus without forming folds or rugæ. But, under the influence of inflammation or other conditions, the peritoneum was deficient in elasticity, and did not shrink co-ordinately with the subjacent muscular tissue, and then it formed ridges and furrows of various kinds. Of these Dr Duncan described three kinds, all of which were seen in the preparations he exhibited.

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[1] Her previous miscarriage took place about four years ago.

The furrows and ridges would of course be in a direction transverse to the direction of the shrinking, and their qualities might indicate the extent of the shrinking, its direction, and its position. The preparations he exhibited showed that this shrinking was not uniform in extent or direction in different parts of the uterine wall.

By accumulation of, and generalization from, observations, valuable knowledge of this important subject would certainly be obtained.

The extent and duration of the shrinking of a uterine wall as a whole might be made out by mere measurement, there being fixed points to measure from. But Dr Duncan knew no other method than this which he now proposed, of discovering or measuring the extents and directions of the shrinking of parts of a uterine wall.

Dr Macdonald said that Dr Duncan had opened up an entirely new field of inquiry, and he hoped one which might by-and-by be fruitful in practical results. As far as he understood the paper, Dr Duncan meant by uterine shrinking the gross effect of, or the summation result of, combined contractions of the entire contractile tissues of the uterus. Now Dr Duncan's observations seemed to him (Dr M.) to be confirmed in this far, that the greatest shrinking, as indicated by the furrows observed by Dr Duncan, took place in the situations in which our knowledge of the anatomy of the uterus would lead us to expect it. Thus, in the body of the organ, the preponderance of muscular tissue was known to be more or less transverse; and here Dr Duncan had observed the preponderance of longitudinal furrows, which would be at right angles to the pull of the transverse fibres; whilst towards the cervix, where we know there exists strong longitudinal fibres, Dr Duncan had noticed that transverse furrows predominated.

IV. *Mr J. R. Hardie* read a paper on PELVIMETRY, and described a method of estimating the conjugata vera, which appears at page 1088 of this Journal.

Dr Duncan said that the Society was indebted to Mr Hardie for his paper; but he should like to point out a deficiency, viz., the absence of any statistics embracing post-mortem measurement.

Mr Hardie said he had confirmed his measurements during life by twenty post-mortem ones.

Part Fourth.

PERISCOPE.

REPORT ON OPHTHALMOLOGY.

BY DR ARGYLL ROBERTSON.

(*Continued from page* 1047.)

CASE OF CRYPTOPHTHALMUS OCCURRING IN A MALFORMED CHILD.

Professor Zehender of Rostock gives a very full and interesting record of a peculiarly malformed child that was brought to his clinique, with the statement that it was destitute of eyes, and of uncertain gender.

The child was six months old, and had been christened Caroline. The parents were well developed, and were not related. This was their only child. The mother stated that, during pregnancy, she had been frightened by having a blind cat thrown at her over a hedge.

The bones of the child's head appeared normally developed, but of eyelids, palpebral opening, or eyebrows, there appeared to be scarcely a trace, the skin of the forehead passing uninterruptedly over the eyes. A shallow groove corresponded to the position of the palpebral opening. This was more marked on the left than on the right side. In each orbit a small movable eyeball could be felt—on the right side more readily than on the left. Careful examination indicated that the child had good perception of light with the right eye, but not with the left.

The child had, further, a large umbilical hernia. The development of the genital organs had been arrested, but the presence of labia majora and a clitoris bespoke the female gender. The sphincters seemed absent, as neither urine nor fæces were in the slightest retained. The three last fingers of the right hand were united together, as were also the thumb and forefinger, and the fore and middle fingers as far as the third phalanx. A similar fusion of the fingers occurred in the left hand, but not to such a degree. The toes were also similarly united, but the big toes of both feet were free.

To enable a photograph to be taken, the child was placed under chloroform, following the administration of which repeated severe attacks of vomiting occurred for several days, which reduced its strength very much. From these attacks it temporarily recovered, but, after a time, they recurred, accompanied by diarrhœa, and induced fatal exhaustion. The child died on the 4th December 1871, aged nine months.

A carefully prepared account of the appearances presented on post-mortem examination is given. The brain was well developed.

In each orbital cavity was found a vesicle, about the size of a hazel-nut, containing fluid. Posterior to this vesicle was situated a rudimentary eyeball. The right eyeball was rather better developed than the left.

The eyes, after being hardened in Müller's fluid, were subjected to examination by Professor Mauz, by whom a very elaborate report of their appearance and structure is given. The chief abnormality was the absence of the lens, the chambers of the aqueous and vitreous humours being separated by an "iris-like" septum, which, however, contained some short curved lens-fibres. In other respects the coats and contents of the right eye did not vary much from the normal. In the left eye there was also atrophy and detachment of retina. With the exception of the orbicularis palpebrarum (which was pretty strongly developed on the left side), none of the true structures of the lids were present—no eyelashes, glands, or lachrymal ducts.—*Klin. Monatsblätt. für Augenheilk.*, 1872. Also recorded in *Report of International Ophth. Congress*, 1873.

DR MORITZ MEYER ON GALVANISM IN BASEDOW'S DISEASE (EXOPHTHALMIC GOITRE).

Four cases of this disease are recorded, in all of which much improvement followed the application of galvanism to the cervical sympathetic.

A weak ascending current was first passed through the sympathetic on each side, and thereafter one pole placed in the submaxillary region, and the other over the closed eyelids or over the goître. These applications lasted two or three minutes.

The first case was that of a girl aged 19, in whom the disease followed cessation of the menses. After fifty-two sittings, the eyes had almost returned to their natural position, and the goître became much reduced, while the menses, which had ceased for almost four years, began to return regularly. After a year of regained health, the patient died suddenly.

The second case was that of a woman aged 38, mother of seven children. After her last confinement she became anæmic, and Basedow's disease supervened. After twenty-four sittings, the goître was reduced to one-third, and the protrusion of the eyes much diminished. After seventy-two sittings, the local manifestations of the disease were almost gone, and the general symptoms materially improved.

In the third case the disease occurred in a female servant aged 30, subsequent to an attack of gastric fever. The exophthalmus and the goître completely disappeared after sixty applications, but palpitation and sleeplessness persisted.

In the fourth case, that of a woman aged 46, there was no enlargement of the thyroid, but a high degree of exophthalmus, with palpitation. In her case eighty-four sittings produced decided improvement.

In all the four cases the disease was more marked on the right than on the left side.—*Klin. Monatsbl. für Augenheilk.*, 1872.

CASE OF ENUCLEATION OF THE EYEBALL, WITH FATAL RESULT.

Dr Just of Zittau records a case of death following enucleation. The patient, a man of 63, had his *left* eye struck by a piece of stone about three months previous to coming under observation. Suppurative inflammation followed, leading to atrophy of the globe and complete loss of sight. The *right* eye had become affected with sympathetic irido-cyclitis. Under these circumstances enucleation of the left eye was performed. Healing went on favourably, though attended with rather more purulent discharge than usual, till the tenth day, when the patient complained of pain in the head, became confused in his speech, in a few hours became comatose, the pulse and temperature rapidly rose, œdema of the lungs ensued, and death occurred forty-eight hours after the first appearance of head symptoms.—*Klin. Monatsbl. für Augenheilk.*, 1872.

DIVISION OF THE CILIARY NERVES FOR PERSISTENT NEURALGIA IN AN AMAUROTIC EYE.

Dr Snellen relates the case of a gentleman who, in 1851, while fencing, had a foil driven through the lower lid and along the floor of the right orbit. After he had recovered from the immediate effects of the injury, his left side was found to be devoid of sensation, and he was unable to move his left leg. His right eye was misdirected outwards, and completely blind, while his powers of smell and hearing on the left side were impaired. Six months later, the patient recovered the use of his left leg, but his condition otherwise remained as before. In 1864 he consulted Dr Snellen on account of excessive sensitiveness to pressure of the amaurotic eye. This existed to such a degree that forcible closure of the lids produced insufferable pain. On careful examination, the region of hyperæsthesia was found to be limited to a sharply-defined spot over the upper and outer part of the ciliary zone. Local applications of atropine and morphia gave only partial relief. In 1871, the patient's condition was not improved, the tenderness of the eye to pressure having rather increased than diminished. Want of sensation in the left arm and leg, deafness of the left ear, and inability to perceive odours with the left nostril, still persisted. The propriety of enucleating the eye was considered; but as the patient was anxious to preserve it, Dr Snellen undertook the division of the ciliary nerves leading to the sensitive spot. The tendon of the external rectus muscle was first divided, and the capsule of tenon opened. Curved scissors were then passed back over the surface of the sclerotic till the projectio of the optic nerve was felt, which was then slightly cut into with the points of the scissors. Considerable bleeding ensued, probably from division of some ciliary vessels. The result of the operation was most satisfactory. The sensitiveness to pressure was completely removed; indeed, in its place there was complete anæsthesia.—*Archiv für Ophthalmol.*, Band xix. Abth. i.

THE EMPLOYMENT OF ANÆSTHETICS IN OPHTHALMIC SURGERY.

Dr Duwez of Brussels strongly advocates the use of anæsthetics in almost all operations on the eye. He much prefers ether to chloroform, as being the safer agent, and because the anæsthesia produced by ether is more constant and complete.—*Annales d'Oculistique*, 1873.

OPHTHALMIC HERPES (HERPES ZOSTER FRONTALIS).

Dr Gosetti relates four cases of this affection. In two, serious ulceration of the cornea occurred, and in them the naso-ciliary branch of the fifth nerve was specially involved. In the other two, the ocular inflammation was trifling, and in them the naso-ciliary branch did not appear to be affected. This is confirmatory of Mr Jonathan Hutchinson's observations. The treatment, ordinarily efficacious in cases of iritis or severe keratitis, was found to produce little effect. The administration of iron and quinine seemed to exert a favourable influence in removing the neuralgic pains that so often persist after the disappearance of the cutaneous eruption.—*Annales d'Oculistique*, 1873 (extracted from *Annali di Ottalmol.*).

Dr Coppez, of Brussels, also contributes a very comprehensive paper on the same subject, giving an excellent résumé of what has been written on the nature and causation of zona ophthalmica. He appends a detailed account of three cases that came under his observation.—*Annales d'Oculistique*, 1873.

CASE OF INTRA-OCULAR ENCHONDROMA OF TWENTY-TWO YEARS' GROWTH.

Dr Chisholm, of Baltimore, relates this very unique case. The patient was a man aged 25. When three years old his left eye was noticed by his parents to be slightly enlarged, and on being examined was found to be defective in sight. Since that time the left eyeball steadily increased in size until he came to Dr Chisholm at the Baltimore Eye and Ear Infirmary for advice. On examination, in place of the left eye a large rounded mass, as large as the fist, protruded from the orbit. The upper lid was immensely expanded and covered the upper three-fourths of the surface of the tumour. The conjunctival covering of the growth was thickened, but otherwise apparently healthy. Under the ocular conjunctiva was an appearance of healthy white sclerotic. All traces of corneal tissue had disappeared, its place being occupied by thick red conjunctiva. From this surface, he stated, serious hæmorrhages had occurred. In the course of its growth, the tumour had caused by absorption an increase in size, and a change in direction of the outer border, of the orbit, and had displaced the nose from its median position. The whole tumour possessed limited movements, synchronous with those of the sound eye. Believing that the eye was the seat of a malignant growth, which from some unexplainable cause had taken on a very irregular and unusual development, its extirpation was advised and assented to.

Enucleation was performed in the usual manner. The tumour came away readily, leaving a huge but apparently healthy cavity, devoid of all fat, and lined with a thickened periosteum. Hæmorrhage was easily controlled by sponge pressure. Repeated hæmorrhages, however, occurred between the fourth and ninth day after the operation, the last of which attacks was deemed so serious as to warrant the tying of the common carotid. Irregular tetanic symptoms supervened, and the patient died on the fourth day after the application of the ligature.

The tumour on examination was found to have on its outer surface the expanded and thickened sclerotic coat of the eye, which had apparently not been perforated by the growth at any point. To this outer surface the muscles of the eyeball were attached. When the tumour was laid open, the whole sclerotic sac was found filled with solid contents, mottled in appearance, and of varied consistence, among which were conspicuous white nodules of different sizes. These proved, on section and microscopic examination, to be cartilage. The tumour was sent to Professor Knapp for examination, whose minute and careful report is given at length.—*Archives of Ophthalmology and Otology*, vol. iii. No. 1.

EXOPHTHALMUS DEXTER, APPEARING DURING INCLINATION OF THE BODY FORWARD, AND DISAPPEARING IN THE ERECT POSITION.

Dr Gruening, of New York, states that the above-mentioned condition was present in the person of a teacher of music, aged 45, who, though weakly built and pale-faced, had always enjoyed good health. The patient stated that about five years previously, while engaged in carpeting a floor in a stooping position, he suddenly experienced a sensation of weight and falling of the right eye. On placing his hand over it, he found that it protruded in reality. He then rose, looked into a glass, but failed to detect any change in the position of the eye, but on bending to resume his work he immediately noticed that the eye was again driven out. Since that time the exophthalmus appears during every inclination of the body forwards, and disappears when the erect posture is resumed. On examination both eyes were found to have a normal range of mobility in all directions—both were myopic—the right more so than the left. With either eye the finest print could be read. There was no derangement of the heart's action or enlargement of thyroid. Whenever the patient inclines his head or body the right eye protrudes. The protrusion increases with higher degrees of inclina and is greatest (measuring 15 mm.) when the patient bends his head forwards and downwards in such a manner as to touch the floor w both hands. A horizontal line drawn across the rather promin bridge of the nose impinges upon the vertex of the cornea. T mobility of the prolapsed eyeball is greatly reduced. It has only a very slight degree of mobility inwards, but absolute immobility upwards, outwards, and downwards. Sight and field of visi remain unchanged. On pressure the eyeball recedes into orbit, b slips out again when pressure is removed. No bruit is heard o auscultation over right temporal region, and no diminution of th exophthalmus is noticed after compressing either right carotid alone or both carotids simultaneously.

Professor Knapp, who saw the case, was of opinion that the protrusion of the eyeball was caused by the gravitation of blood in the pathologically cavernous retro-ocular tissue of the orbit.—*Archives of Ophthalmology and Otology*, vol. iii. No. 1.

SYPHILITIC MEMBRANOID OCCLUSION OF THE RIMA GLOTTIDIS.—In the January number of the *American Journal of Syphilis*, Dr Louis Elsberg has a paper on syphilitic membranoid occlusion o the rima glottidis. The notes of six cases occurring in his own practice, and five collected from other sources, are given. In 270 cases of laryngeal syphilis coming under Dr Elsberg's observation, membranoid occlusion of the rima occurred six times. He considers the disease a tertiary manifestation, and a local termination of the syphilitic process. The web is formed of cicatricial tissue, and appears always to begin at the anterior angle of the rima. The treatment Dr Elsberg recommends is both local and constitutional. If the patient has already had much mercury, iodide of potassium should be given, otherwise the two may be combined. "In every case of extreme dyspnœa we must be ready to perform tracheotomy: *this may be needed before we can attempt to remove the web.*" To divide the web, Dr Elsberg uses the galvano-cautery. As the conducting tubes or rods must be very thin, and the platinum point white hot, the heat rapidly spreads towards the handle. To obviate this it is necessary closely to surround the rods with silk thread, and dip the instrument into cold water before introducing it.

ON THE SULPHIDES OF POTASSIUM, SODIUM, AND CALCIUM.—Professor Sydney Ringer calls attention (in *Lancet* of February 21, 1874) to the value of these drugs in abscesses, boils, and scrofulous sores. In glandular abscesses of children, after measles and scarlet fever, the use of one-tenth of a grain, with a grain of sugar-of-milk every hour or two hours, seems to diminish the size and hasten the suppuration of the swellings to a very marked extent. In boils and carbuncles, it seems equally useful, diminishing the area, liquefying the core, and assisting in the discharge of the sloughing cellular tissue. Dr Ringer recommends the use of belladonna and glycerine over the neighbouring parts round the boil, or, still better, that these should be protected by a belladonna or opium plaster spread on leather, with a hole cut in it as large as the boil, which should then be smeared with glycerine and belladonna, and have a poultice spread over all. The indolent scrofulous sores so common in children are also amenable to treatment by the sulphide of calcium. They dry up, reappear less frequently, and even in those more serious cases in which the bone and periosteum are affected, the general health is improved, the pus becomes more healthy, and the sore heals.

In acne rosacea and acne indurata, good effects are gained by the external use of the iodide of sulphur ointment of the Pharmacopœia. It should be thickly smeared over the eruption night and morning. Even in bromic acne the good effect of this treatment is noticed.

M. CHARCOT ON MENIÈRE'S DISEASE, OR VERTIGO AB AURE LÆSÂ.—*Le Progrès Médical*, 24th and 31st January, contains extracts from a clinical lecture delivered by M. Charcot at la Salpetrière, on a very interesting case of "Menière's disease," or "Vertigo ab Aure Læsâ."

The patient G. is about 51 years of age, and remains in a constant state of vertigo, which is aggravated by the slightest movement. Her feeling is that of being placed on the top of a high tower without the protection of a railing, or that of looking over a precipice. This vertigo continues almost without intermission night and day, and in all postures. The erect posture, however, increases it enormously. Any movement also conveyed to the patient—such as suddenly tilting the couch on which she lies—aggravates the vertigo, so as to cause her to cry out.

At intervals, in the midst of apparent calmness, and without any perceptible cause, she suddenly starts. In explanation of this starting, she states that in addition to the habitual vertiginous state, she has paroxysmal exacerbations of the vertigo. These appear to be characterized by the sensation of sudden movement of the body of the patient—not of surrounding objects—which is, however, entirely subjective, and only indicated externally by the starting. Consciousness remains unimpaired, and the patient can afterwards describe what she has felt.

Most frequently she fancies that she performs a somerset either forwards or backwards. More rarely she experiences the sensation of rapid rotation of the body on its vertical axis, the rotation being invariably from left to right.

These motor hallucinations are invariably followed by great anxiety, pallor of the face, and cold sweats. Nausea and sometimes vomiting terminate the attack, and the vertigo then returns to its ordinary level.

The patient has—and this is a point of great importance in diagnosis—whistling in both ears, but especially and more markedly in the left ear. This becomes at times so intense that she confounds it with the whistling of a steam-engine at the neighbouring railway station.

This exacerbation of the ordinary whistling sound is important in this respect, that it always announces the near approach of a vertiginous attack with the somersets.

The ears were examined by an expert, and the following points were brought out:—1st, There has for a long time been a discharge of sanious pus from both ears, but particularly the left. 2d, In the right the membrane of the tympanum is thickened and covered with greenish deposits; in the left it has disappeared and been replaced by fungoid growths. On this side the hearing is very imperfect.

The typical symptoms of Menière's disease, which are somewhat obscured in this case, owing to the aggravated form which the disease presents, may be clearly made out in the antecedent history. The first symptoms appeared thirty-four years ago, when the patient was seventeen years of age. She was then seized suddenly with an affection of the left ear, characterized by shooting pains, which often interfered with sleep. Sanio-purulent discharges were frequent at that time. She was for a long time under the care of Menière.

Attacks of vertigo—at first rare and not severe—gradually became more pronounced and frequent. Between the ages of twenty-five and thirty-eight she frequently felt an intense humming in the ears, and then her chair seemed broken in pieces under her. She gave a cry, rose quickly, and the attack was at an end.

About the age of thirty-eight the humming was replaced by acute whistling, and nausea and vomiting began to form part of the attacks.

These came on frequently in the streets, and the patient was obliged to support herself against the walls to avoid falling forwards. When at home she was in the habit of assuming a position in which the head was thrown back, and the legs slightly elevated. By this means the attacks were rendered less severe.

About the age of forty-five the attacks became so frequent as to be almost incessant.

She was shortly afterwards admitted into "la Salpetrière," under M. Charcot, and has since remained in the state described at the outset.

M. Charcot holds that this disease, which is of pretty frequent occurrence, is not properly understood, and is often mistaken for apoplectiform cerebral congestion, apoplexy, petit mal, and, above all, gastric vertigo, and he admits that the diagnostic difficulties occasionally encountered are very great.

He draws special attention to the following points, which will, he thinks, render the diagnosis comparatively simple.

Humming or singing in the ears is of common occurrence. It is the intimate relation which exists between their sudden development or aggravation, and the invasion of vertiginous sensations which is the point of diagnostic value in Menière's disease.

The aural bruits which precede the vertigo in this disease are so intense that they have been compared by patients to "the whistling of a steam-engine," "the rattling of a bag of nails," or "the noise of firing," etc. This bruit occurs solely or principally in one of the ears. In slight or recent cases it ceases with the vertiginous attacks; in severe cases it merges into the habitual slighter form of humming or singing.

Erelong more or less marked and permanent deafness comes on, coexisting with labyrinthic otitis, or it may be central otitis with anchylosis of the ossicles propagated to the vestibule and labyrinth, or it may be a simple catarrh of the ear.

It is quite possible, indeed, from what we know, that any pressure whatever on the tympanum, conveyed by the chain of small bones to the labyrinth, may bring on the symptoms of Menière's disease.

The vertigo itself consists of a sensation of movement of the whole body from behind forwards, or from before backwards, so as to describe a fall forwards or backwards, as the case may be. There may be in addition a feeling of rotation round a transverse axis—a veritable somerset. Sometimes the rotation seems to be round a vertical axis, either from right to left or from left to right. Some patients experience these different kinds of motion at different attacks. The point to be noted is, that these sensations are entirely subjective. Their only outward expression is a start, movement of surprise, grasping at surrounding objects for support, or sitting down to escape from falling.

The patient remains perfectly conscious throughout the attack, and can afterwards recount his experiences. Nausea and vomiting almost always occur at the termination of an attack. During the attack, the face is pale, the skin cold and covered with sweat, resembling syncope rather than apoplexy. There may be more or less severe transient headache. There is never any difficulty of speech, muscular spasm of either face or limbs, tingling, numbness, any sensation resembling an aura, or temporary paresis.

M. Charcot reserves for a future occasion the discussion of what is known regarding the pathology and probable nature of the disease, and concludes with a few hints on its prognosis and treatment.

It is remarkable that, as a rule, those serious lesions of the nerve-centre, which so commonly result from disease of the internal ear, do not usually occur in even the most aggravated forms of Menière's disease. The deafness gradually increases and finally becomes absolute, and when this is the case the vertigo and whistling gradually diminish, and finally disappear. This fact has suggested the question, Might not operative interference hasten this desirable event in such melancholy cases as that of the subject of the lecture? M. Charcot expresses no opinion on this point. The best-directed treatment often fails. Where the vertigo is connected with aural catarrh, it may sometimes be relieved by treatment directed to the catarrh. In a case observed by M. Hillairel, the vertigo ceased completely after the opening of an abscess of the internal ear. The application of energetic counter-irritation is recommended.

FORK-SWALLOWERS.—"L'homme à la fourchette est le héros du jour." This seems the opinion of the French press, and the case itself is of interest and has given the excuse for the recital of numerous similar cases. He is a lad of 18, a shop-boy, who had been fascinated by the tricks of a juggler who swallowed swords, knives, and forks. The last trick he had imitated a dozen of times, when one day the fork slipped from between his teeth and could not be got hold of. A doctor caught its end with a pair of dressing forceps, but the attempt at removal gave the patient so much pain that he had to let go, the fork slipped into the stomach, and the pain was at once relieved. M. Labbé, under whose care the patient is, has proved the presence of the fork in the stomach, by an examination by a special sound under the influence of chloroform. The sound has a sounding-board apparatus attached to it. No attempt at interference had as yet been made.

M. Baillarger has reported a case in the *Union Médicale* of a lunatic who swallowed a fork, and lived six years with it in his stomach. He had walked with a peculiar stooping gait, and tried to avoid sudden shocks. It was found at the autopsy with the points at the cardiac end of the stomach.

M. Loscoles de Lozere tells M. Labbé of the case of a lady, who in 1824 swallowed a fork, which was well borne for six months. It began to cause pain and swelling in the stomach; and eventually was cut out by M. Quairoche with perfect success.

M. Le Tellier reported, in the *Revue de Thérapeutique* in 1853, the case of a suicidal female, who, after various unsuccessful attempts, managed to swallow a fork. It gave no trouble for four years, when intense pain and abscesses in the thigh heralded its appearance after a long journey from the stomach. The greater part of it was removed from the trochanteric region, but the patient died exhausted.

Dr Charpy of Lyons saw the following:—A female lunatic, æt. 56, swallowed a fork, told of it, was not believed, till after some days a tumour was felt in the right iliac fossa, which increased in size, became red, with adherent skin, eventually bursting and giving issue to a large fork twenty days after it was swallowed; for fifty-five days air and debris of food came through the wound, which eventually healed up.

M. Dureau has written to the *Gazette Médicale* an interesting and elaborate note of remarkable substances found in the stomach. From this we extract the following bearing on the fork question:—Mr Lund's case of recovery after passing a dessert-knife from an abscess close to the umbilicus is an allied case. Dr Chemin's case, reported by Velpeau to the Academy of Sciences in 1849, was that of a man, who, trying to force down a veal bone which had stuck in his throat, swallowed the fork by accident. It remained fifteen days in the stomach, four months in the pyloric end, three months in the small intestine, and eventually was passed by stool much diminished in size. The patient recovered.

M. Foville records at great length, in the *Gazette Hebdomadaire* (No. 18, 1874), two rather curious cases in which one maniac swallowed *seriatim* the whole of a set of dominoes with which two old gentlemen had been playing, while they were taking a short walk in the garden, and passed them all again by stool; while another, in a state of religious fervour, swallowed his beads, to which a cross was attached, and passed it also entire without injury.—*Various French Journals.*

Part Fifth.

MEDICAL NEWS.

Livingstone Missionary Memorial.—We are glad to see that, after the last funeral obsequies have been rendered to the great African explorer, and an admiring nation has placed his remains among those of her choicest sons, the Medical Missionary Society of Edinburgh have devised a mode of perpetuating his memory, which, to our mind, is altogether in accordance with the character, the life-long purposes, and history of the man. The idea is, to erect an edifice in every way adapted for a Medical Missionary Dispensary and Training Institution, and, if practicable, on the site of the old premises, which are not only inadequate as to accommodation, but in an uncomfortable and semi-dilapidated condition; and to designate this new building by the title of the *Livingstone Memorial Institution.* We can imagine no better mode of doing honour to the great medical missionary than by thus endeavouring to train men who shall carry forward the enterprise of which Livingstone was a remarkable pioneer. That he was a medical missionary, in the ordinary acceptation of the term, we do not aver; because his toils and hardships as an explorer were necessarily too engrossing; but still he was a medical man and a missionary in the highest sense, for the faith which animated his manly soul looked beyond all physical sufferings and present anxieties, to a bright and glorious future when "Ethiopia shall yet stretch forth her hands unto God."

We gather from the pages of a contemporary that there is an impression abroad that medical missionaries, as a rule, are "stronger in their theology than in their surgery." This notion may have arisen from the tendency which clerical missionaries sometimes exhibit, with a mere smattering of medical knowledge, to attempt rashly to cure the bodily ailments of those who come under their spiritual care. We willingly concede that the temptation to do so is strong; for the two occupations have a natural alliance, and are associated together by Scripture precept and example. Still, so far as the students of the Medical Missionary Society are concerned, the allegation is totally unfounded. They all receive a thorough professional education, either at the University or at the Extramural School; and if one lesson in the Training Institution is pressed upon them more emphatically than any other it is this, that a medical missionary is nothing, and worse than nothing, if he falls short in surgery.

The undertaking will require a considerable sum of money; but we doubt not that many members of the medical profession will gladly put a stone into the edifice which is meant to commemorate, in a manner so appropriate, one of our brethren distinguished above most for his heroism, his scientific labours, and his Christian philanthropy.

Dr A. G. Miller, F.R.C.S.E., 15 Shandwick Place, and John Lowe, F.R.C.S.E., Superintendent of the Mission Home, 56 George Square, act as treasurers.

THE following opinions, by Lord Cockburn in his "Memorials," on Edinburgh Doctors will be read with interest:—

17*th October* 1846.—Dr John Thomson died on the 11th instant, at his villa of Moreland near the Grange, to which he had retired from all business a few years ago. He was eighty-two. He began life as a weaver, in Paisley; and ended it, after extensive practice, as a retired Professor in the University of Edinburgh, and the most learned physician in Scotland.

To almost his last week he was a hard student, and not even fourscore years could quench his ardour in discussing science, morals, or politics. How he enjoyed a dispute! He was a discerning and attached patron of youthful and friendless merit, and was rewarded by his fruits. There are men who owe their rise to him, and who bless his memory, all over the world. Though he did nothing in 1793 and 1794 but express his Whig opinions boldly, this was enough to bring him within the proscription of that reign of terror, and to fix him in his principles, which he was most useful in promoting through all his subsequent life. He was one of the marked men of that resolute and public-spirited class which is now rapidly disappearing, and to which Scotland owes so much. His peculiar usefulness arose neither from his talents, his learning, his warmth of heart, nor his steadiness of principle, but from his enthusiasm. He never knew apathy, and medicine being his field, he was for forty years the most exciting of all our practitioners and of all our teachers. Was a right principle, or a right institution, or a right man in danger—especially, was any of them in danger from indifference—get Thomson. He made them blaze. Men, especially young men of promise, were inspired by his zeal and his confidence in the triumph of truth. The dullest professor was made ashamed, or, if this was impossible, was properly abused. The most brutish town-council was defied and spoken true of. John Thomson was always the star.—(Pp. 163–5.)

16*th December* 1847.—An eminent Edinburgh man is dead in London—Robert Liston, surgeon. He was a native (I believe) of the county of Linlithgow, but was educated and began his practice as an operator and a lecturer in Edinburgh. After establishing a great character here for professional skill and jealousy, he removed to London about ten or twelve years ago, and soon rose very high, till at last he was in as great reputation, and in as extensive practice, as any other man in the operating line. He was about fifty-three years old. The taste for quarrelling with his brethren, which obstructed him so much in Edinburgh, did not desert him, though I believe it abated a little in the wider field; and in both places, in spite of this too common medical weakness, his general goodness of heart secured him considerable popularity and many friends.—(Pp. 201–202.)

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

SIR,—Allow me to correct an error which has crept into your report of the Obstetrical Society Meeting of 25th March.

In page 1042, I am made to say, "That however fat a horse became, its arteries did not grow atheromatous." The remarks I made in answer to Dr Macdonald were, "That mares very rarely suffered from parturient apoplexy, no matter how old or how fat they might be at the time of parturition." Now, it is well known that the bloodvessels of the horse are far more frequently the subject of morbid changes than are those of the ox; hence, if this argument of Dr Macdonald were correct, it would naturally follow that the disease would be more often seen in the mare than in the cow. I do not look upon blood-pressure as *the* cause of parturient apoplexy in the cow, but, as a not by any means inconsiderable factor in its production. That the blood-pressure is much greater in the cow than the horse is an undoubted fact; and this, with other collateral causes, was fully considered by me in an essay read before the Edinburgh Veterinary Medical Society, and published in the *Veterinarian* for November 1872. Albuminuria, I am inclined to think, is as often an effect as a cause.—Yours faithfully, THOMAS WALLEY.

PUBLICATIONS RECEIVED.

Dr M. Bernhardt,—Die Sensibilitäts-Verhältnisse der Haut. Berlin, 1874.

J. B. Budgett, M.D.,—The Hygiene of Schools. London, 1874.

Dr Socrate Cadet,—Intorno l'Efficacia del Solfuro Nero di Mercurio. Rome.

Catalogue of the Library of the Surgeon-General's Office, U.S. Army. Washington, 1873.

William J. Corbet,—On the Statistics of Insanity. Dublin, 1874.

Dental Surgeon,—The Dental Profession. London, 1874.

William Eassie,—Sanitary Arrangements for Dwellings. London, 1874.

John Eric Erichsen, F.R.C.S.,—On Hospitalism. London, 1874.

Guy's Hospital Reports, vol. xix. London, 1874.

Henry Mac Cormac, M.D., Arrangement of Houses. Belfast, 1874.

F. W. Pavy, M.D.,—Treatise on Food and Dietetics. London, 1874.

Report of the Autopsy of the Siamese Twins. Philadelphia, 1874.

Third Annual Report of the Board of Health of City of New York. New York, 1873.

Fifth Annual Report of the State Board of Health of Massachusetts. Boston, 1871.

Thirty-fifth Report on Prisons in Scotland. Edinburgh, 1874.

Annual Report of the Supervising Surgeon of the Marine Hospital Service of the United States for 1873. Washington, 1873.

Alexander Silver, M.D.,—Practical Medicine. London, 1874.

Richard S. Sisson, M.D.,—The Simple or Non-Mercurial Treatment of Syphilis. London, 1874.

W. Domett Stone, M.D.,—An Epitome of Therapeutics. London, 1874.

Sir Henry Thompson, F.R.C.S.,—Cremation. London, 1874.

T. E. Thorpe and M. Pattison Muir,—Qualitative Chemical Analysis. Lond., 1874.

PERIODICALS RECEIVED.

American Journal of Insanity,—October, January.

American Journal of the Medical Sciences,—October, January, April.

American Journal of Obstetrics,—August, November, February.

American Journal of Syphilography and Dermatology,—October, January, April.

American Practitioner,—Nov. to May.

Annales de Gynécologie,—January.

Annales d'Oculistique,—September to April.

Annali di Chimica applicata alla Medicina,—October to December.

Archives Générales de Médecine,—January to May.

Archives de Physiologie,—March to May.

Archives de Tocologie,—Feb., March, May.

Australasian Medical and Surgical Review,—October to February.

Berliner Klin. Wochenschrift,—Oct. 27 to May 18.

Birmingham Medical Review,—January.

Births, Deaths, and Marriages, Monthly Return of,—October to April; Quarterly Return,—September, December.

British and Foreign Medico-Chirurgical Review,—January, April.

British Medical Journal,—November 1 to May 23.

Bulletin Général de Thérapeutique,—Oct. 30 to May 15.

Chicago Journal,—January.

Detroit Review of Medicine,—March, April, May.

Dublin Journal of Medical Science,—Nov. to April.

France Médicale,—October 25 to May 16.

Gazette des Hôpitaux,—Oct. 23 to May 21.

Gazette Hebdomadaire de Médecine, etc.,—October 31 to May 22.

Gazette Médicale de Paris,—October 25 to May 16.

Glasgow Medical Journal,—January, April.

Indian Medical Gazette,—October to April.

Journal de Médecine et de Chirurgie,—Nov. to May.

Journal de Thérapeutique,—January 10 to May 10.

Journal of Anatomy and Physiology,—Nov.

Journal of Mental Science,—Jan., April.

Journal of the Scottish Meteorological Society,—October.

Klinische Monatsbläter für Augenheilkunde,—September to March.

London Medical Record,—Oct. 29 to May 20.

Lyon Médical,—October 26 to May 10.

Medical Press and Circular,—October 29 to May 20.

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